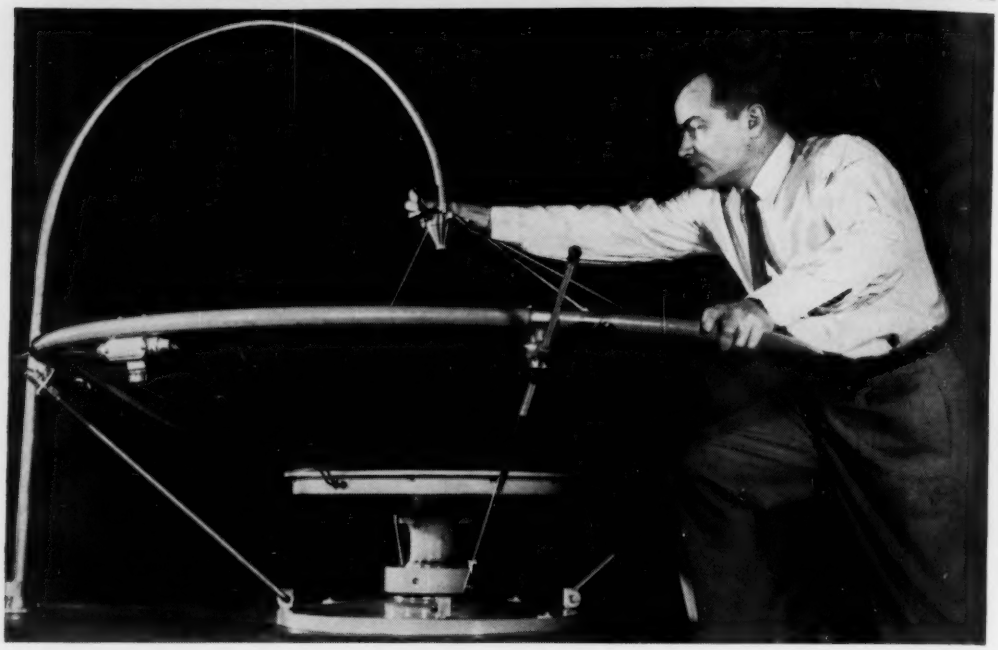
The SCIENTIFIC MONTHLY

VOL. 83 NO. 4 OCTOBER 1956

TEACHING A GIANT TO TAKE SHORT STEPS



Bell Laboratories' Dr. J. W. Fitzwilliam adjusts a waveguide feed to a parabolic dish reflector. Dr. Fitzwilliam, who has a Ph.D. in physics from Massachusetts Institute of Technology, leads the practical development of Bell's new 11,000-mc. system. Components had to be developed to operate in a frequency band not previously utilized.

The giant microwave highway that carries your TV programs along with telephone conversations from coast to coast has a versatile new partner—an entirely new microwave system which was created, and is now being developed, at Bell Laboratories. The new system operates at 11,000 megacycles—a much higher frequency than ever before used in telephone service.

Bell's present microwave systems—operating at 4000 megacycles—were designed for heavy traffic and long distances. The new system is designed especially for lighter traffic and shorter distances—up to 200 miles. Its traffic capacity is extremely flexible. Depending on traffic needs, the system can provide only one one-way or as many as three two-way broadband channels. Each two-way channel can carry 200 telephone conversations simultaneously or one television program in color or black and white in each direction along a route. The new system, which is already being operated experimentally, will be particularly valuable in providing additional telephone service and TV programs for cities in remote areas.

This is another example of how research and development work at Bell Laboratories help the Bell Telephone System to serve you better.

Mr. L. C. Tillotson, who originated the new system, adjusts the klystron-isolator combination which made the system feasible. Mr. Tillotson, an M.S. from the University of Missouri, heads research in microwave applications.

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Science and Technology

Radiation Meter

A new radiation survey meter makes possible direct readings of x-ray, gamma, and beta radiations ranging from 3.0 to 3000 mr/hr, on a single scale. The time constant of the portable logarithmic survey meter is inversely proportional to radiation intensity, which minimizes reading time and personnel exposure in fields of high-intensity radiation. A one-tube circuit provides an accuracy of ± 20 percent through the entire range, and a built-in current source eliminates the need for radioactive sources to calibrate the unit. (Atomic Instrument Co., Dept. SM, 84 Massachusetts Ave., Cambridge, Mass.)

Electronic Thermometer

Rapid temperature measurement on an indicator that is calibrated in Centigrade and Fahrenheit degrees is permitted with a thermometer that utilizes the thermistor principle. The probe is sterilizable. (Tri-R Instruments, Dept. SM, 24-15 44 Rd., Long Island City, N.Y.)

Variable Reactance Instrument

A variable reactance instrument for the 8200 to 12,400 Mcy/sec frequency range, has been developed. Rated at 300-kw peak power in matched line, the instrument is used to introduce a standing wave of desired magnitude and phase in a waveguide; it operates directly at any power level up to the breakdown power level of the waveguide. The magnitude of the standing-wave ratio in the line can be varied from 1.02 to 2.0 by means of a micrometer adjustment on the top carriage. The phase of the standing wave is separately variable by the movement of the carriage. Phase adjustment is greater than half a guide wavelength at the lowest frequency. The phase scale offers direct reading to 0.5 mm with vernier reading to 0.05 mm. (Narda Corp., Dept. SM, Mineola, N.Y.)

Laboratory Stirrer

Designed for general laboratory use, a new stirrer can also serve as a shaker drive for Van Slyke apparatus and separatory-funnel extractions. The instrument operates with speeds from 100 to 1200 rev/min in 11 discrete steps. The motor is controlled by a tapped autotransformer. (Gerald K. Heller Co., Dept. SM, 1 N. Carey St., Baltimore 23, Md.)

Still

Mixtures in the molecular weight range of 200 to 700 are separated in a new still that features a glass column with an air-cooled rotary condenser and a dry-ice trap. In operation, distillate coming from the agitated surface of the pot is condensed by the rotating condenser and thrown to the upper, warm evaporation surface where, under controlled conditions, it is re-evaporated and again condensed in the rotating condenser. Capacity of the still is 2 lit, and vacuum range is 0.1 μ-Hg. (Arthur F. Smith Co., Dept. SM, Rochester 3, N.Y.)

Liquid Cooler

A new surface cooler prevents vaporization and splashing of liquids that are under agitation in a blendor. It operates by stabilizing the agitated liquid at its surface where cooling is most effective. Vigorous agitation of the liquid in the blendor forces it up and down the outside of a cooler bulb through which water is circulated. Surface contact cools the external liquid, thus reducing or eliminating vaporization. The metal sphere maintains the contents of the blendor at 1° to 2°C above the temperature of the circulating water. (Central Scientific Co., Dept. SM, 1700 Irving Park Rd., Chicago, Ill.)

Data Processor

A data processing system that uses 12-decimal digit numbers is capable of 11,000 additions, 2000 mutiplications, or 13,000 comparisons per second. The electronic system's high-speed internal "memory" may be supplemented with up to 10 magnetic drum storage units, each having a 12-million-decimal digit capacity, and a 4-msec access time. Multiple tape units provide enlarged lower-speed storage. Input-output consists of punched cards, line printer, high-speed paper tape, and electric typewriter, as required. Operating strictly in decimal mode, the instrument is constructed on the modular principle, allowing compatible expansion for increasing data-processing requirements. (Logistics Research, Inc., Dept. SM, Redondo Beach, Calif.)

Probe-Type Detector

A scintillation detector features a preamplifier circuit that permits its use with quarter-volt scalers and ratemeters or gamma-ray spectrometer systems. The instrument, designed for medical, biologic, and physical research where gamma-emitting radioisotopes are used, also contains a gamma-sensitive sodium iodide crystal, a 6292 photomultiplier, magnetic shielding, and a detachable nose-piece collimator. The preamplifier provides a gain of ten with negative output for use with a normal scaler or ratemeter and a gain of two and cathode-follower output for use with a spectrometer. Bandwidth is sufficient for 1-μsec resolution. Over-all detection efficiency is 50 percent for iodine-131 and 45 percent for cobalt-60. Background is less than 200 count/min when the instrument is used with scaler or ratemeter and 2 to 25 count/min when it is used with a gamma-ray spectrometer system that is set on isotope photopeak. (Nuclear Instrument and Chemical Corp., Dept. SM, 229 W. Erie St., Chicago 10, Ill.)

Silicone Rubber Compounds

A recently published 8-page brochure provides, in addition to specifications of silicone rubber compounds, data on the properties of silicone rubber in general, as well as its resistance qualities under a variety of conditions. (Raybestos Manhattan, Inc., Dept. SM, Passaic, N.J.)

THE
SCIENTIFIC MONTHLY

OCTOBER 1956

Science, Humanities, and Artifacts

HARCOURT BROWN

Dr. Brown has been professor of French language and literature at Brown University since 1937. He has long been interested in the historical relationships between science and the humanities, especially in the mutual interferences in the 17th and 18th centuries. He has written articles on several aspects of this topic, a monograph on the Académie de Physique de Caen (1664–1670), *and* Scientific Organizations in 17th-Century France. *This article is a slightly revised form of a paper read at a symposium on "Science and the Humanities" that was held 29 Dec. 1955 during the Atlanta meeting of the AAAS.*

THE latest edition of the *Pensées* of Blaise Pascal (*1*), that strange and incomplete final work of a strange genius, mathematician, physicist, and Christian apologist, places the following paragraph at the beginning of his book.

"How hard it is," he writes, "to propose something for another's consideration without corrupting his judgment in the very manner of proposing it! If one says, 'I find this beautiful; I find this obscure,' and so on, one draws the imagination towards this view, or provokes the contrary. It is better to say nothing; and then the other judges according to what he is, that is to say, what he is at that moment, and according to what other circumstances, for which the speaker is not responsible, will have put into him." And Pascal adds that even silence is accompanied by movements and facial expressions which lead to different interpretations, "so difficult is it not to throw judgment off its base, or rather, so infirm and unstable is the foundation of man's judgment."

Few paragraphs illustrate quite so clearly the hesitations and embarrassment of a mind trained in the sciences as it approaches the characteristic realm of the humanities. Pascal at this time had largely given up the study of physics and mathematics and was entering on the long and difficult preparation of his *Apology for the Christian Religion*. In order to defend the position of his family and others associated with Jansenism and the Sisters of Port-Royal, he had to think in theological and exegetical terms, and he seems to have perceived at once that the scope and purpose of his new activity demanded a change in style and method as well as in scale and content. In the interests of vigorous and persuasive communication, many of the qualities of scientific style—objectivity, impersonality, economy of statement and argument—would have to be sacrificed.

Those Pensées that are devoted to eloquence, to man's rank and position in the universe, and to such themes as the proofs of immortality, suggest vividly the intellectual distress produced by a change of standards, by a shift in the criteria of truth and validity in argument. Pascal was uncomfortable in the presence of a new variable, the reader whose emotional attitude is to be influenced, who is not convinced by exact argument on the basis of generally accepted conventional values. He found that historical fact was not as easily discovered and stated as were the qualities and quantities of the natural world, that moral good and religious truth were not as quickly and accurately measured as was the height of a column of mercury. His dilemma, in short, offers an opportunity and an instructive pattern for the exploration of a boundary that is still troublesome, still a matter of debate whenever scientists and students of the humanities meet to define their purposes and reach toward a new synthesis.

Challenge of the Artifact

The studies included in the humanities, as I understand them, are all, in one way or another, devoted to the discovery and investigation of artifacts, with a special convergence of attention on the artificer. On some levels—for instance, that characterized by arrowheads, scraping tools, and campfire ashes—we must be content with reconstruction of the culture and typing the men responsible in fairly comprehensive classes—Woodland, Algonquian, and so forth. However, whoops of delight are heard whenever a bit of carving, a daub of crude paint, or an original modeling in the potsherd allow the archeologist to go beyond the class or type, to sense the presence of an individual, to find a human being under the mass of statistics and rubbish left by the tribe or clan. This jubilation is the mark of the humanist. He has found a man; a mind and will, an intention, a volition, a purpose, with its retinue of discriminations, of value judgments and personal reactions, where so far there had been nothing but data. Now ends and values, idiosyncrasies and uniqueness, all the traits of the rocky irreducible individual, begin to blunt the tools of science. We have come to something we cannot further subdivide, something which in the long run cannot be adequately or completely classified.

In other words, the problems we face in the humanities are not always, indeed they are rarely, susceptible of resolution by strictly scientific methods. When we speak of books, even as literature, we may think of physical objects, bindings and papers, formats, printer's ink, type, which we can analyze by means of chemistry or by study of the mechanics of production. The scholar who neglects the results of skilled analysis of these material features does so at his peril; there are certain celebrated forgeries of English poetry to warn the student of the risks that may be run by those who neglect the sciences and insights of the expert bibliographer (*2*).

On the other hand, the scholar who might hope to estimate the age or textual value of a document, its relative place in a historical sequence, by the exclusive use of such methods would be making a very great mistake. Accurate dating of printed matter, books, pamphlets, or broadsides, often defies scientific analysis. Paper, especially that made of rags, is very durable, and type fonts and ornaments can be used for many years after they are first cut and cast. Furthermore, the arts of the printer and binder change so slowly that 10 years is a minimum, and a quarter of a century a much safer, margin of error to allow if dated testimony is lacking. Priorities cannot be surely established by circumstantial evidence alone; a witness with a date is better than all the clues that physics or chemistry or even a Geiger counter can reveal. The positivist scientific approach to these problems yields approximations only, logical probability at most, which always is second best, after the authority of the written or printed word, always provided that the word is honest and undeceived (*3*).

For example, among innumerable undated plays of the Golden Age in Spain, humanistic analysis of the content, even as carried on by a relative amateur, has shown itself successful where the positive methods of competent scholars had produced fairly satisfactory approximations. Skillfully devised and generally accepted techniques of the paleographer and bibliographer had been used on the documents, printed and manuscript; vocabulary and style, versification and stanza form, and the structure and thematic content of the plays were analyzed with assiduous care, and an acceptable chronology was suggested. There remained, however, a lingering doubt that the results really permitted the recognition of a sequence on which one could base a definitive history of the Spanish theater or a biography of the author; there was always a chance that his development had not really followed the curve of the statistics, that he used verse and stanza forms at random, or that he had archaized in language from time to time. Here again, internal evidence turned the probabilities of the scientist into certainty; an outsider in professional and academic circles succeeded in setting up a convincing chronology based on recognizable features of acting companies in operation, the presence in successive plays of a pair of growing children, and the career of a tame and senescent lion. On such evidence we students of the humanities are inclined to rest; when we have a complete record in the documents, there is no need for nonliterary, "scientific" approximations (*4*).

There are other less valid reasons for the humanist's tendency to disregard, perhaps to underestimate, the value of scientific methods and evidence. Most of us smile when it is pointed out that a recent book on linguistics contains a chapter which proves by mathematical analysis that "the German language has improved 211% in the last fifty years," which seems to be the only tangible result of much original, and perhaps misguided, investigation. There has been much use of statistical methods in these areas recently; their value depends on the discrimination used in sorting the raw material and on the recognition of the strictly limited purposes for which the results obtained may be used. It may be interesting to discover the "native

ontent" of Milton's language, the archaisms in a Romantic author, to test methods of linguistic analysis, or to establish rough chronologies among different literary works as I have suggested. But on the whole, we are happier when the scientific linguist leaves the literary languages alone and devotes himself to pidgin English and Eskimo. Discovery of what an author means is one thing; the analysis of raw material may be useful at times, but it must be performed within the context of his artistic pattern, with due regard for intentional effects, such as the use of vulgarisms, or archaic or overrefined diction as an illustration of character.

Thus the humanities begin with careful and imaginative study of the artifact, the reading of the book, the progressive discovery of the meaning of the total work through perceptive analysis of parts and function. The goal is creative synthesis, a painful endeavor to recapture the full purpose of the author. Whether we are studying literature, music, painting, or any other art, our effort is to find and participate in the point of view from which he worked; to discover whether his purpose was narrative or expository, persuasive or contemplative, philosophic or practical, and the degree to which these purposes were consciously mingled. We seek the postulates from which his work sets out, the method, intellectual or artistic, by which he proceeds, the criteria he uses where choice is possible, and the end for which the whole was made. We are chiefly interested in the *why*; we may often ask the scientific *how*, but no humanist can exorcise the teleological *why* for very long.

It is apparent therefore that, in the humanities, classification and distribution into genera and species, taxonomy, can be achieved only by a high degree of abstraction, really by mutilation and distortion of the evidence. Furthermore, even the most complete history of one of the arts—of literature, for instance—while it may remain as a conscious objective of many humanists, the rallying point of all these studies, can never substitute acceptably for the reading of the literature itself. The aspects of literature, and probably of any of the arts, of which independent historical sequences may be established, are innumerable, certainly not reducible to a single series, or even to a manageable cluster of series. A new book on any important period or aspect of one of the arts may complement, but it will only very rarely entirely supersede, its predecessors. Sooner or later, we inevitably go back to earlier interpretations for understanding, perhaps even for forgotten facts. Audubon can still tell us something about birds, if not as a scientist, at least as an artist; his relationship to his subject-matter is kin to that of the humanist toward the works of art he studies.

Documents or Data?

In an early discussion of scientific method, in the *Fragment of a Preface to a Treatise on Vacuum,* Pascal pointed out that there are two chief areas of human inquiry, those deriving authority from documents, and those seeking understanding of the external world. If we add to written documents the data obtained from critical examination of artifacts—interpreting this term as broadly as possible—we may continue to accept this rough division and define the humanist scholar as one who discovers and discusses the truth implied in objects produced by human workmanship. His authority is another statement made by a human being, on paper or parchment, in words; on canvas, wood or stone; in music or gestures, and so forth. The humanist cannot make one group of facts into something else without falsification; his material is given to him in its physical substance, in its context, as well as in its form and pattern. Through it he seeks an ever deeper understanding, a continually enlarging view of what its author intended. The control over what he sees there lies in the degree to which he can persuade his colleagues and posterity of the validity of his insight.

In all this work there is little resemblance to the procedures of science. The humanist may claim that he is engaged on an endless quest, the rolling back of a constantly expanding frontier, but the center and boundaries of it are both subjective. What works of art yield to the critic, when he turns to Michelangelo, Beethoven, Montaigne, Cézanne, or Jean-Jacques Rousseau, is limited only by his own inventive capacity; there is no prospect of finality in the research in progress on such men. Pascal believed that the authority of books could be described as limited, in contrast with the infinite complexity of nature; he would be profoundly disillusioned if he were to look at our expanding libraries and consider the difficulty any librarian faces when he tries to discover books that he may justifiably discard. The difficulty lies in the innumerable vistas opened by a single moderately simple statement when it is interpreted by half a dozen reasonably sophisticated humanists who look at it in its historical and cultural context, its precedents and its influences.

This kind of intuitive understanding is not easily defined, but it seems to be quite different from that of the scientist. Not a scientist myself, I have to use scientists' definitions as a means of clarification, and to be brief I propose two that seem useful. One

occurs in *The Scientific Monthly* (5), where "understanding" is said to take place when a particular phenomenon can be regarded as illustrating a more general law; the instance cited is that Newton's theory of gravitation permitted physicists to "understand" Kepler's model of the solar system. I doubt that any physicist would allow it to be said that any student of the humanities understands anything in his special field, for in three decades of humanistic study, I have never met the kind of relationship between law and experience that this scientist speaks of. Many "laws" have been suggested, but they operate pedagogically in classrooms rather than in the world of creative literature.

I take another formulation from the conversation of a distinguished physicist whose colleague I have the honor to be. For him, "understanding" occurs when he can take an idea into his laboratory and do something with it; when it suggests an operation, an experiment, a process, an arrangement of related parts which can be duplicated and thus lead to a result previously unknown, permitting a deeper penetration of the structure of relationships that compose the natural world, the discovery of regularities unrecognized before. Once more, my experience with literary studies fails me; ideas can be brought home and applied, but their value is more often not a value of differentiation, adding distinctions and individuality to what is on hand rather than regularities and resemblances.

In short, I cannot accept the distinction which S. Giedion attributes to Turgot, the 18th century economist and statesman, that "the sciences are immense, as nature is; the arts, which are only relationships with ourselves, are limited, as we are." There seems to be no reason to regard the number or diversity of qualities possible in the arts as less baffling, or less interesting, or even less important, than those found in the sciences. Both are constructs of the mind; they have different purposes, different principles, and different products, and it is no reflection on either to suggest that they are incommensurable.

A Book Is Not a Textbook

If the scientist is justified in doubting whether a humanist may be said to understand anything in the way a scientist does, what may we say the humanist is trying to achieve? There was a man who divided all intellectual activity into two great branches, theoretical physics and collecting postage stamps; he was not a philatelist. The humanist seeks accurate and more and more complete description, but not for purposes of classification. As I have pointed out, the humanist is not usually a taxonomist, and Polonius' listing of the kinds of plays that Hamlet might enjoy—"tragedy, comedy, history, pastoral, pastoral-comical, historical-pastoral, tragical-historical, tragical-comical-historical-pastoral, scene individable or poem unlimited"—raises eyebrows or a smile but no criterion. The humanities are not really normative; evaluations are spontaneous and intuitive, not susceptible, except in the vaguest and most general way, to rules. The rationalist Voltaire found that the only bad *genre* was the *genre ennuyeux*. In his *Candide* he gives us a specimen of that product of the most reasonable of styles, the learned author who knows the rules, who can recite the pattern of the perfect play, but whose work can never hold the stage because it is so dull. The *je ne sais quoi*, the unknown quantity or quality which no classical critic could ever successfully define, is the indispensable component, the ultimate necessity in every work of art. With it, any irregularity is finally condoned; without it, no perfection of order, no elaboration or ornament can mask the weight of essential dullness.

The goal of the student of the humanities is therefore not the discovery of laws of art in general, or the elaboration of formulas by which the raw material of a culture, its music, its painting, its sculpture, and its literature may be neatly relegated to oblivion while the textbook takes the place of contact with originals. The physicist does not absolutely have to know the creative books on which the fabric of his science is erected; time has selected the crucial experiments, rewritten the formulas, rephrased the laws he needs, and the scientific supply house can offer him the materials for a rerun of the tests any time they are needed. The blood, sweat, and tears of the great discoverers can be forgotten. But Swift and Voltaire, Raphael and Donatello cannot be learned from a history book; only hours spent in assiduous study of the original, in laborious reconstruction of the particular experience which the book embodies, can convey the meaning or meanings of *Gulliver* or *Candide*, or the *School of Athens*. The history is a short cut, an abstraction in which it is hard to find the experience of the flesh and nerves of the creator whose activity it records. A guide to the subject, a compendium of information, may be memorized, but it offers no basis on which a new art, a new literature may be founded. In itself, history may be a creative activity too; Xenophon, Thucydides, Caesar, Gibbon, Voltaire, and Toynbee produce works of art, whose study may be regarded as a humanistic pursuit; but here, again, all the considerations just set forth apply as before.

Reduced to objective scholarship, then, the

humanist sees himself rather as a conservative, concerned with the retention, the preservation, and comprehension of the past, the promotion of the understanding of other cultures, remote in time and space, of other forms of human expression. He tends to see science as a major agent of social change, "its silent appropriation of this dominant function," as Lord Balfour writes, "the most vital of the revolutions which have marked the development of modern civilization." Whether this is a fair description of science or not, it is a conception that bulks large in the minds of most students and teachers of the humanities. They see in science the chief enemy of the traditional cultures, the more dangerous because of its very intentness on progress in techniques and immediate results, without regard to the longer trends, or to the ultimate purposes in the culture and civilization as a whole. Paul Sears may write that it is not the destiny of science to ease man's labors or prolong his life, or to serve the ends of power, "but to enable man to walk upright, without fear, in a world which he at length will understand, and which is his home," but there lingers a doubt whether all the engineers and technologists understand this fully, and a fear lest there be some misunderstanding about our home, which may become uninhabitable if some of the sciences advance faster than, and unaccompanied by, understanding of man himself.

The difficulty seems to lie in the increasing tendency of modern scientific man, not necessarily the scientist, to forget what our world is like without him, without his works, his highways and airplanes, his radio, and his highly efficient machinery of destruction. Most of the liveliest sciences today are remote from the human scale of measurements, of cause and effect. The range between erg and dyne, angstrom unit and meson, on one hand, and parsec and megaton on the other, is vastly greater than a Pascal could visualize three short centuries ago, and yet it is in this range of magnitudes that reasonably exact measurements and predictions are possible. But plants and animals are within the frame of reference of everybody; and the weather, sickness, and death, the comforts and conveniences of the common life, even the automobile and the bathtub, retain the element of hazard, of fortuitous coincidence, and continue to demand human, and humane, qualities of everyone. Science works on this level too, with its statistical regularities, but within the actuarial framework there is enough uncertainty about the individual destiny to allow us to delight by preference in the intuition of the artist, the tragic sense of a Racine, the cosmic vision of a Milton, the depth and diversity of a Molière, a Shakespeare, or a Dante, which none of the humanistic sciences can explain away.

Integration and False Analogies

All this, of course is not intended to deny that we are all in one academic boat and breathe a common atmosphere. The influence of science on the scholarly study of the humanities has been considerable, and much of it has been good. While a scientific vogue in the 1880's led French theorists to discover evolutionary patterns in the history of lyric poetry, the novel, and the theater, a keener sense of what was scientifically possible soon caused the rapid discard of such analogies and a return to more accurate descriptions of what had actually happened. The presence of scientific standards of truth has developed the sense of fact, enlarged the capacity to criticize theory and hypothesis, and let much of the wind and water out of our work. I think it safe to say that the change in outlook among literary humanists, and perhaps among historians of art, esthetics, and religion as well, over the last 200 years is quite as marked and follows many of the same lines as those found among social scientists and general historians. We are all concerned to avoid predicating occult powers and faculties; we strive for a plain direct manner of thought, and we try to use understood and accepted methods to obtain results that can be demonstrated visibly or audibly to our colleagues.

There are other aspects of our activities toward which the methods and objectives of science have not been quite so beneficial. There has been a perhaps inevitable trend toward seeking immediate results by high specialization, not only among the broad fields of world literature, but within the various phenomena of a single culture, and even within the narrow limits of a single method of analysis. Still claiming to be humanists, scholars have become adepts in the trivial, confining their attention to bibliography, to the sociology of a particular period of a national literature, to ethnology, psychology, the new criticism within a narrow application, neglecting the general principle that a truth can be established among natural phenomena only if its base is wide. All this has been undertaken, of course, in the belief that it is useful to seek positive results in a small field, using a few sharply defined criteria on a few phenomena, rather than a broad philosophic perceptiveness to meet the challenge of the arts and literature as a whole, in all their diversity and richness.

The fault in this narrowing of outlook is due partly to the increasing complexity of our culture, and partly to human timidity toward general ideas

that are not sheer platitude. It is always easier to make up one's mind about Pascal's style than about his religious views or his value as a guide to man's nature. Likewise, one cannot expect a teacher concerned with linguistic facts and biographical details, which are more easily absorbed and remembered by many of our undergraduates, to have much time for the identification and discussion of the great intellectual themes, the tensions between religion and science, body and soul, ethics and the passions, in the authors being read. Yet there is no doubt that these large issues of meaning, these questions of the *why,* are much closer to the heart of artist and author. There is a long path from the details of grammar and prosody, vocabulary, syntax, sources and influence, to the full understanding of what the author means. Unfortunately, the path is not always followed very far.

Discovery of the Individual

Thus we return more and more closely to the vigorous formula of Hippolyte Taine, "Rien n'existe que par l'individu; c'est l'individu lui-même qu'il faut connaître." We believe this, and we turn away when Taine neglects this outlook in the adopted science of his theory of *race, milieu et moment,* which blights the introduction to his *History of English literature,* and tells us that vice and virtue are products, which may be discussed like vitriol and sugar. Unreservedly to accept this view is to deny the need for discrimination of ends and purposes, for diversity of methods, for a sense of personality. Taine himself is far from being reduced to a single standardized method and jargon; he has a strong individual insight into the books he reads, the poets, novelists, dramatists, and critics he discusses. We are able to neglect his science of history, the rigorous analysis of abstractions which turn men into statistics, and delight in his intuitive and spontaneous response to the richness of the world's art and literature.

What has happened to Taine illustrates at least one of the attitudes of modern humanists toward the development of science in recent times. The formulas that propose an analysis of the author, such as Taine's race, milieu, and momentum, are neglected as science and taken as historical events, the data of our study. They become products of their own coordinates, to be filed away among innumerable other samples of 19th-century positivism, an interesting sample, with little value as we recreate the historical flow. Taine's science becomes the subject of research, a special and unique activity, a product of a personality rather than of biological and environmental factors. Like other events in the history of science, it is in many ways unique, capable of being understood and evaluated in many different terms, from different points of view. The humanist may try to see a scientific activity as the scientist sees it, or he may legitimately see it in relation to any one of a number of parallel activities, to which it may contribute, from which it may derive, or toward which it may be antipathetic and harmful. He may, at some peril to the integrity and cogency of his main interest, neglect the contribution that a knowledge of the historical development of science may make to his special study; but this peril is no greater, or less, than would be a neglect of the knowledge of the history of theology, politics, or morals. It is even possible that the humanist component is the most necessary ingredient of studies in the history of science.

The situation in which the humanist scholar finds himself is well illustrated by the final pages of A. R. Hall's recent book, *The Scientific Revolution, 1500–1800* (*6*). The general issues of his book are all clear; Hall states with little fear of contradiction that "compared with modern science, capitalism, the nation-state, art and literature, Christianity and democracy, seem regional idiosyncrasies, whose past is full of vicissitudes and whose future is full of dark uncertainty," and he adds that modern science sprang from a society which lasted some 400 years, but which is now in dissolution, and which it is most likely to survive. Unfortunately, it is the historical question that puzzles Hall. Science emerged from this intermediate historical stage, he writes, "and it is this emergence that we do not adequately comprehend." Two full and final pages develop the questions he finds he must ask; to a humanist they appear very familiar indeed, exact parallels of those we have been asking and partly answering these many years.

These questions all spring from the human factor, the element of uncertainty that is inevitable as soon as the unpredictable individual appears on the scene. Why do men commit themselves to one kind of proposition about nature rather than to another? Why is one type of statement more plausible than another? Why talk of corpuscles rather than of spirits without factual evidence for either? Why all the talk about vacuum? Thought was modified in ways that cannot be ascribed to the results of observation and experiment; historical comprehension demands psychological and philosophical insight, and a command of historical facts which is not now available, "for," he adds, "the operation, the incidence, and the impact of the creative intellect are almost unknown." In con-

clusion, he reflects that "we cannot write the full history of science save by reflecting the operations of original thought, which we do not understand; and . . . we cannot exclude from science, which is rational, the influence of factors which are irrational."

Fruitful Differences

It is a little hard to summarize the principal elements of the study of the humanities. It seems to be essential to expect that there will be a recognition of the presence of the individual man in every investigation, even though this admits an element of subjectivity. This must, of course, be modified by an ironic perception of the limits, not only of one's own competence, but also of the subject matter; and these limits are in themselves also relative. The humanist necessarily recognizes that he may be wrong, even in talking about his own field of interest. If he is to be accurate in his surmises, he must study the human being at the heart of any artistic or intellectual creation; and he must recognize that the unity of knowledge, if it is to exist usefully as an explanation of the phenomena, depends on the discovery of unity in man himself rather than in the external world.

Somebody, my notes do not say who, has recently written that James Thurber's *Let Your Mind Alone* "is about seven times as educational as anything James Harvey Robinson ever wrote, because Thurber has digested his culture; it has turned into Mr. T."

I suggest that the best literature, and perhaps the best humanistic scholarship, is that in which science and the other components have been completely absorbed, "have turned into Mr. T."; in other words they have penetrated the whole structure, contributing their proper share of understanding and perceptiveness, a sense of relatedness to the general range of organized experience. Science, among other things, appears thus in the best of Voltaire's tales, in *Zadig,* and more especially in *Candide;* in Flaubert's *Simple Heart* as well as in *Madame Bovary;* in a modern novel like Albert Camus' *La Peste* as well as in Pascal's *Pensées.* The digestive process is difficult and not always complete; in such a type as science fiction, it is not uniformly successful. Much literature and art, and many humanists, reject the component completely, or leave it in a state resembling its original condition, an uncomfortable lump, a burden to the senses and the spirit.

Neither scientist nor humanist should be surprised, therefore, if there is an inevitable lag between the arrival of a new science, a new technique, a new theory, or even a new world-view, and its general and fruitful acceptance and use by humanists; there is a similarly comprehensible delay before a new outlook among humanists and artists is recognized by scientists. One can sympathize with the impatience of those who seek immediate synthesis of the two outlooks, but it may be suggested soberly and conscientiously that we are all better off for the presence around us of great diversities of outlook, methods of argument and investigation, and objectives. Disharmony of method and outlook will remain as long as scientist and humanist remain true to their callings, as long indeed as they remain men. All we can legitimately hope for is that we may each achieve, in the realm of intellectual objectives as elsewhere, by means of constantly renewed effort, the comprehension and mutual confidence that grows where differences are recognized and encouraged for their fruitfulness.

References and Notes

1. Edited by Louis Lafuma (Paris, 1952), p. 96, No. 1.
2. The story of the forgeries associated with the name of Thomas J. Wise has been retold often enough. See especially J. Carter and G. Pollard, *An Enquiry into the Nature of Certain XIXth Century Pamphlets* (London and New York, 1934), and also W. Partington, *Thomas J. Wise in the Original Cloth* (London, 1946). The latter book is revised and enlarged from an earlier American publication, *Forging Ahead.*
3. Useful as the Geiger counter has been in achieving approximate dates of organic remains, its lack of complete precision makes it less useful as the prehistorian or archeologist approaches the earliest monuments datable in other ways. Henri Frankfort, recently director of the Warburg Institute, has said that he was disappointed with the best results carbon-14 could produce in dating early Egyptian remains. The objectives and methods of the humanistic bibliographer have been clearly and perspicuously stated by L. C. Wroth in an essay, "The bibliographical way," *Colophon,* n.s., **3**, No. 2, 225 (1938), reprinted in *About Books, a Gathering of Essays* (Univ. of California Press, Berkeley, 1941), pp. 79–102.
4. T. Wilder, "Lope, Pinedo, some child-actors, and a lion," *Romance Philol.* **7**, 19 (1953–54).
5. M. J. Walker, "An orientation toward modern physical theory," *Sci. Monthly* **81**, 28 (1955).
6. A. R. Hall, *The Scientific Revolution, 1500–1800* (Longmans, London, 1954).

History is simply a piece of paper covered with print; the main thing is still to make history, not to write it.—BISMARCK.

New Interpretation of the Surface of Mars

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LAST month the planet Mars passed closer to the earth than at any time since 1939. On 7 September it was only 35 million miles distant, but for several weeks before and after that date it was well placed for observation.

Probably no other celestial object ranks with Mars in popular interest. This stems partly from the suggestion made many years ago that the system of "canals" had an artificial origin (*1*). But even if no such features had ever been discovered, there undoubtedly would be much speculation about possible inhabitants. Next to the moon, Mars is more favorably situated for settling, by observation, the question of whether or not it supports life. Unfortunately the general public has been treated to many more articles by pseudo scientists, wishful thinkers, and romanticists, than by well-informed writers and astronomers or other competent scientists. We need only recall the Orson Welles broadcast on H. G. Wells' *War of the Worlds* to appreciate what a hold "Martians" (as "really existing" creatures) have had on the imagination of many people within the last two decades. Considering the recent and continuing "flying saucer" agitation, it is unlikely that the situation is at all improved.

On purely scientific grounds, however, Mars should have a wide appeal. The meteorologist should find material of great interest in the moving clouds as clues to the atmospheric circulation as well as in other atmospheric phenomena. The composition of the surface materials might now be better understood had the problem been attacked by trained mineralogists. The forms of areas of contrasting colors could present to the geologist a challenging problem of interpretation. The "canals," whatever their true forms, also may have a partly geologic interpretation, whether as surface deposits or as traces of deeper crustal structure. The seasonal changes of color of certain areas have long been quoted as evidence of possible vegetation, so that here is something of interest to the botanist.

Geologic Processes on Mars

It might be expected that astronomers and astrophysicists would have exhausted all approaches to the Martian problem that are possible with the evidence now at hand. It is my thesis that they have missed one of the most promising approaches, simply because they have not seriously attempted to interpret the surface geologically. This is not to imply that a geologist can surely give a correct interpretation. It does mean that without the serious application of geologic thinking, there is little hope of solving the Martian problem.

Consider briefly the more prominent geologic processes and the possibility of their occurrence on Mars, in the light of what we know of conditions there. In Table 1 a number of geologic agents and processes are listed at the left, and at the right is a brief statement concerning their operation on Mars.

Reasons for most of the statements in the right-hand column are probably obvious. Mars and the earth have had very different histories, because of the extreme scarcity of water on Mars. The judgment against orogeny may require some comment. Although the mechanism is not understood, it is clear that on the earth all the folded and thrust mountain structures have formed where marine sediments accumulated miles thick in sinking troughs, or geosynclines. Without fluvial erosion and marine sedimentation, probably there can be no such thick accumulation on Mars. Conceivably,

a greatly modified orogenic process might occur on Mars. If we grant the possible formation of sinking segments by some unspecified mechanism, then aeolian deposition might form the thick sedimentary series, and compression might be relieved by crumpling in that region. Probably the results would differ radically from those we see on the earth. The possibility of volcanism is discussed in later paragraphs.

Within the limits of our terrestrial experience, the only widespread processes that we can envision on Mars are chemical weathering, exfoliation, aeolian erosion and sedimentation, dislocation by faulting, and volcanism. Areology is much less complex than geology.

We sometimes encounter the statement that Mars' surface has been *eroded* to low relief. This is closely related to the widespread, but unwarranted, tendency to think of Mars as "older than the earth." Granted that the relief is low, it is an open question whether erosion or sedimentation has brought about the condition. Since aeolian processes are dominant, quite likely the level surface has resulted from the filling of hollows by wind-deposited materials.

The foregoing considerations led me to attempt an explanation of Martian surface features along entirely new lines. The salient points can be summarized as follows. (i) A pattern of bands and streaks appears to be oriented in directions that resemble those of the expected prevailing winds. (ii) Some bands cross the equator and curve in directions that agree with the deflection of winds by the Coriolis effect. (iii) Dark markings appear to fan out from small spots, as if these were local sources of dark dust. (iv) It was inferred that these sources are active volcanoes whose ash is carried by the winds and deposited to form the visible pattern. Later sections of this article elaborate the details of this hypothesis.

Table 1. Geologic processes on Mars.

Agency or process	Operative on Mars?
Rain	No
Snow	Doubtful; surely very little
Frost	Yes, observed
Fluvial erosion and deposition	No
Wave erosion	No
Marine sedimentation	No
Talus formation; gravity sliding	Very minor, if present
Ice wedging	Very minor or absent
Glacial erosion and deposition	No
Chemical weathering	Yes; perhaps slow
Exfoliation	Yes; if solid rock is exposed
Wind transportation and deposition	Yes!
Action of living organisms	Quite surely minor, if present
Gravity faulting	Probable
Orogeny (folding and thrusting)	Unlikely
Volcanism	Possible; strong indirect evidence
Artificial operations	Almost certainly *no*

Dimensions, Motions, Seasons

Before passing to the interpretation of the Martian surface, let me review the established facts concerning the planet. This is only a general summary; for a more extended and critical discussion the reader is referred to the excellent treatment by deVaucouleurs (*2*).

Mars has a diameter of about 4200 miles, only slightly more than half that of the earth. Its mass is known accurately from the motions of its two small satellites and is 0.108 of the earth's. From these data, the mean density is 4 grams per cubic centimeter and the surface gravity is 38 percent of that of the earth. The polar flattening is extremely difficult to measure directly and would remain quite uncertain, except for the perturbations of the satellites by the equatorial bulge. The oblateness determined by this dynamical method is 1/192. If this is accepted, we can draw some conclusions concerning the internal distribution of density. Mars is certainly more homogeneous than the earth. If it has a denser central core at all, it is proportionally much smaller than the earth's.

The Martian year is 687 of our days, and the period of its axial rotation is 24 hours 37 minutes. The axis is inclined 24.8 degrees from the perpendicular to the orbit plane, nearly matching the value for the earth (23.5 degrees). Therefore the northern and southern hemispheres must experience seasonal variations of insolation similar to those on the earth. An added complication is introduced by the eccentricity of the orbit, 0.093, as against the earth's 0.017. The planet passes perihelion during the summer of the southern hemisphere, and the insolation at vertical incidence then exceeds that of the opposite season and hemisphere by about 45 percent. Therefore, the southern hemisphere must experience greater extremes of temperature than the northern. The southern summer will be short and relatively warm, the northern longer and much cooler. Since the average color of the southern hemisphere is darker than the northern, the range of temperature of the ground surface will be further accentuated.

Atmosphere

Mars has an atmosphere. This is indicated by a veiling of features near the limb as well as by the appearance of moving clouds. Photographs taken in ultraviolet light usually fail to record the surface of the planet at all, so great is the scattering power of the atmosphere (*3*). The various attempts to measure the amount of atmosphere are summarized and critically discussed by deVaucouleurs (*2*, p. 124). The most reliable result appears to be that of Dollfus (*4*), which is based on measures of polarization of the scattered light. If the scattering efficiency of the Martian atmosphere for visual light is equal to that of our air, the total mass per square-centimeter column is about 22 percent of that in the earth's atmosphere. Owing to the lower surface gravity, the pressure at the ground is only 83 millibars.

Concerning the composition of the atmosphere, we have only a little direct information. Kuiper (*5*, p. 335) identified carbon dioxide and estimated the amount to be about twice that in the terrestrial atmosphere, per square-centimeter column. A later discussion by Grandjean and Goody (*6*) yields a higher value, about 13 times, but in any case it remains a minor constituent. No other gas, not even water vapor, has been found. For both water and oxygen, it is estimated that as little as 0.2 percent of the terrestrial amount would have been detected if present (*7*). Concerning the main constituent of the atmosphere, we have no direct evidence. From considerations of cosmic abundances and the probable loss of chemically inert gases in the early stages of accretion of the planet, we naturally surmise that nitrogen forms the bulk of the atmosphere. This cannot be checked spectroscopically, since molecular nitrogen has no absorption lines in the observable region of the spectrum. Argon formed by the decay of radioactive potassium should also be present. If we assume that, area for area, the same amount of argon has been produced as on the earth, then the relative abundance of this gas must be about 5 times that of the earth. The hypothetical composition of the Martian atmosphere is then 95 percent nitrogen, 5 percent argon, and small fractions of a percent of carbon dioxide, and perhaps neon, krypton, and xenon.

Clouds observed on Mars appear to be of two kinds. Bluish high-level clouds are believed to be comparable with our cirrus and are tentatively considered to be composed of ice spicules. Yellowish clouds are frequently seen at lower levels. These seem to be great duststorms, and in some cases they have been observed to move for hundreds of miles across the face of the planet.

Surface Temperatures

Temperatures at the surface of Mars have been determined from observations with sensitive thermocouples (*8*). The image of Mars at the focus of a large reflector is caused to fall upon a minute thermojunction. The temperature can be calculated from a measure of the thermoelectric effect of all the received radiation as compared with that which passes through a water-cell. The main source of uncertainty lies in the correction for absorption in the earth's atmosphere. The essential results are in agreement with what we should expect: Mars is colder than the earth, as it should be at its greater distance from the sun, but, in the equatorial regions shortly after noon, the temperature averages several degrees above 0°C (*9*). The daily temperature range is large, for the surface that has just emerged from the night side of the planet has temperatures of −70°C and lower. From this it is clear that the atmosphere has relatively little blanketing effect, probably because of the nearly complete absence of water vapor. The temperatures of the dark areas of the surface appreciably exceed those of the lighter reddish areas (*10*).

Observation of the Surface

When Mars is nearest the earth, its diameter subtends an angle of only 25 seconds of arc. Thus, under a magnification of 1000 times, it would appear to the observer about the size of a large orange held at arm's length. The theoretical limit of resolution of the 200-inch telescope is 0.023 second, and this would very rarely be realized, owing to turbulence in our own atmosphere. Hence, under the most ideal conditions, we might hope just barely to resolve two spots 4 miles apart. For the largest refractor, the Yerkes 40-inch, the theoretical limit would be 20 miles. Already the prospect of determining much about the nature of Mars' surface looks rather hopeless.

The surface of Mars is plainly visible in ordinary light of the visual region and can be photographed with great clarity in the red and infrared. There is a wealth of detail, too complicated to reproduce completely by the slow process of sketching at the telescope and too fine to record photographically. It is doubtful that the accomplished resolution on any photograph has ever approached that which is possible to the eye within a factor of 5 or so. In order to obtain an instantaneous exposure, the image must be small, and the finest details are utterly lost in the plate grain. If the image is preenlarged by means of auxiliary lenses, then its scale and reduced light intensity necessitate lengthened

exposures during which the image is blurred by turbulence in our atmosphere. It is this circumstance that has hampered the collection of objective data to settle such controversial questions as the nature of the so-called "canals." Recent experiments with electronic magnification offer some hope of improving the clarity of photographs.

Features of the Surface

Even a small telescope will show the principal types of areas on the disk of Mars. The white polar caps are the most conspicuous features, except when they are approaching minimum size. The greater part of the surface has an ocherous hue, which gives the integrated light of the planet its familiar reddish tint. Finally, a number of dark markings are usually visible. At first sight their forms may seem irregular, but in the main the most conspicuous ones occupy a belt south of the equator and project in places into the northern hemisphere, where one or two large isolated dark spots are also found. Parts of these dark areas display changes of tint as the Martian seasons progress, and upon this phenomenon is founded the suggestion that they may be covered with some form of vegetation. From analogy with the dark areas of the moon, the term *maria,* or "seas," has been used for these regions, although it has been obvious for many years that they are not bodies of water.

With higher telescopic power, one can make out a number of smaller diffuse dark spots sprinkled pretty much at random through the reddish "desert" areas. These were named "oases" originally, and the name has been retained without implication that they are any less dry than other parts of the planet. The controversial "canals" are often seen with telescopes of 6-inch aperture and larger. Many observers have recorded them as more or less linear features. To some they appear as extremely fine lines; others see them as diffuse, but still rather straight, streaks. Some of them are represented as intersecting at oases, and where they connect with the "seas" there are usually more or less definite "bays," or indentations, in the "coast." Under high magnification with instruments of more than 30-inch aperture, the canals are lost. There are many indications that they represent a merging of fine detail in instruments of low resolution or low magnification, and that high magnifying power and adequate resolution separate the details of the pattern. It is true that some observers with fairly large telescopes (for example, Trumpler [*11*] with the 36-inch Lick refractor) have recorded many canals, but these observations were made with relatively low magnification that did not use the full resolving power of the telescope. One of the most able of all visual observers, E. E. Barnard, never saw any straight and narrow canals, although he attempted many times with the Yerkes 40-inch refractor. The most extensive series of observations with a telescope larger than 30 inches in aperture was carried out by Antoniadi (*12*) with the Meudon 33-inch refractor, and he only occasionally recorded a feature that had some resemblance to a canal. Instead, he saw many of the canals resolved into irregular, blotchy, natural-looking detail. This result has been confirmed by Dollfus (*13*) with the 24-inch refractor at Pic du Midi under conditions of superb seeing.

One sometimes sees the statement that "the canals have been photographed," and from this it is argued that they (that is, the *fine, narrow lines*) exist. The original statement is of the nature of a half-truth, and the argument from it is specious. Let me state the case more objectively. Photographs have been taken, upon which features of a generally linear character can be seen in the locations of the canals as mapped by visual observers. All that the photographs and the "visible canals" on them prove is that human perceptions are subject to the same fallibility and illusions, whether we view Mars directly through the telescope or look at a photograph. Without doubt the subjective impression of the canals is based on some features that have objective existence. Neither the visual observations nor the study of photographs have yet revealed the precise nature of the real features. The canals have been discussed critically by Kuiper (*14*), along approximately these lines.

Topography; Physical Nature of the Surface

The surface of Mars appears to be rather smooth. Certainly there are no ranges of high mountains. If any topographic features like those of the moon existed there, they would be conspicuously visible, even though the atmosphere would tend to soften the shadows. But considerable differences of elevation are not ruled out if the slopes are relatively gentle. There are reasons for suspecting that some regions are high plateaus, because they are chronically subject to deposition of frost or snow. But isolated mountain peaks of considerable height are not impossible. A conical peak a mile high and with the characteristic slope of a volcanic cone would be a difficult object to detect, even with the 200-inch telescope. Conceivably, peaks more than 2 miles in height have been missed. On the other hand, ridges as long as 50 or 100 miles and only 3000 feet high could hardly have escaped notice. This dis-

tinction between isolated peaks and long ridges is worth a good deal of emphasis. For many years, textbooks and even technical articles have quoted uncritically a statement originally made by Lowell (*15*), to the effect that no mountains in excess of 2500 feet in height could have been missed. The original statement was made without qualification, but examination of the assumptions on which Lowell based his calculation shows that he tacitly assumed mountains of lunar type—that is, many tens of miles in extent—and not sharp, isolated peaks.

We can be fairly confident that no permanent bodies of water exist on Mars, and it is very doubtful that even small ponds exist temporarily. It was long ago pointed out that any water surface near the center of the disk should give a solar reflection that would appear as an intense point of light. The position of such a reflection is calculable for any date, from the known relative positions of earth, sun, and Mars. No such spot has ever been seen in the expected position. On very rare occasions brilliant spots have appeared briefly (*16*) in such locations that they might be interpreted as sunlight reflected from sloping surfaces coated with glare ice, but such interpretation is purely conjectural.

That the polar caps are actually water snow, and not dry ice, is established by their infrared reflection spectrum (*5*, p. 337). The caps grow and shrink annually in the manner we should expect from the inclination of Mars' axis and the consequent seasonal changes of temperature. They are probably very thin—a few inches at most, except for their more permanent central parts, which may be somewhat thicker. The shrinking polar cap is clearly visible as a rule; the forming one is usually enveloped in haze, under cover of which it grows to its maximum dimensions before becoming clearly visible as an extensive white surface. Probably the disappearance is chiefly a process of sublimation; it is unlikely that any actual body of water is formed in the process. At the most, the surface soil may become slightly damp.

The general surface of the planet is reddish, and it was natural to compare it with the Painted Desert. It has commonly been suggested that the coloring material, as in the case of red sandstones, is hematite. This was linked with the absence of oxygen; it was suggested that all of Mars' oxygen has been used up in oxidation of the surface rocks. The reflection of light from the "deserts" does not differ greatly from that of red sandstone in the visible spectrum. But in the infrared, Kuiper (*5*, p. 335) found that the reflection did not agree with that of hematite. Rather unexpectedly it was found that the closest match was given by a brownish felsite. More recently, Dollfus (*17*) has suggested, on the basis of polarization studies, that the coloring matter of the "deserts" is finely powdered limonite, iron hydroxide, the same mineral that colors many of our terrestrial soils.

It was found by Lyot (*18*) and confirmed by Dollfus and Cailleux (*19*) that the variation of polarization of the reflected light with changing phase angle is closely similar to that shown by volcanic ash. This does not necessarily mean that Mars is actually covered with dust of *volcanic* origin; any fine dust would produce a similar effect. But in all probability the surface is a very dusty one, whatever the source. The moon's surface shows a similar effect. In this case, the dust can be attributed to the effect of numerous impacts of meteorites. The greater part of the Martian dust is probably the product of weathering and, as we shall see later, a good case can be made for volcanic ash.

Since carbon dioxide is present, and since there is at least a trace of moisture (the polar caps *are* water), chemical weathering can occur, however slowly. We have no reason to doubt that the minerals of the Martian surface are the same as those we know, perhaps in different proportions as a result of different geologic history. Chemical weathering alone would produce a fine powder, and the winds could distribute this over the whole surface of the planet. This would account for the polarization, without invoking volcanism. This point is emphasized, because several astronomers have made the mistake of assuming that I jumped to the conclusion that volcanoes exist on Mars because Lyot and Dollfus had mentioned volcanic ash as giving the best match to the observed polarization. Actually, my suggestion of volcanism (*20*) was based on completely independent evidence.

Dark Areas: Vegetation?

The dark areas, or "seas," have long been pointed out as areas that may harbor a vegetable cover. The greenish tinge seen by a number of observers suggests this; the seasonal changes of tint appear to reinforce it. Earlier ideas involved the assumption that the dark areas were actually marshy tracts, and it seems to have been conjectured that the plants were comparable to some of our higher forms. Spectroscopic observations, however, are quite conclusively against any close similarity. In 1937, Millman (*21*) determined the spectral reflection of the dark areas and found that it differed only slightly from that of the bright deserts. The dark "seas" appear relatively darker in longer wave-

lengths and reflect more strongly in the blue and violet. However, it is difficult to disentangle the light actually reflected by the surface from that scattered by the atmosphere above it. Probably much of the violet excess of the dark areas is of atmospheric origin.

Chlorophyll has a strong reflection in the yellow-green. Millman's results showed that this maximum was definitely not present in the light from the dark areas. He, therefore, expressed doubts concerning the presence of vegetation in those areas. In retrospect it is surprising that such doubts had not been stated many years earlier. It is well known that leaves of trees and grass have a very high reflectivity in the infrared. Positive prints of infrared negatives show trees appearing nearly white. Since the Martian "seas" appear very dark (*3*) in these wavelengths, there is no support in these observations for the vegetation hypothesis.

More conclusive results were obtained by Kuiper (*5*, p. 339) with a lead sulphide cell. He found that the "seas" were uniformly darker than the "deserts," with no indication of a stronger reflectivity in the infrared. From this he concluded that higher types of plants must be absent. However, he pointed out that the reflection from some lichens and mosses is much weaker in that region, and that all forms of vegetation could not be ruled out. His statement was made with all due caution and was so carefully worded that misinterpretation appears inexcusable. Nevertheless, it has suffered serious distortion. Kuiper's statement that the data were "not inconsistent with" the presence of such low forms as lichens has been twisted to become "consistent with" and next to become "proof of." It is worth repeating, for emphasis, that Kuiper gave no hint that the observations *suggested the presence* of any vegetation. He merely stated that the data *did not demonstrate its absence.*

Recently Tikhov (*22*) has summarized work by himself and a number of collaborators on the spectral reflection of plants at high altitudes and low temperatures. He states that the infrared reflection of some of them becomes low under conditions of extreme cold. If this is correct, then some of the objections to higher plant forms on Mars would be removed or much reduced.

Dollfus' (*17*) studies of the polarization of light reflected from the dark areas gave results quite similar to those in the red "desert" regions. Apparently, both light and dark areas are covered with fine dust that could be either volcanic ash or the products of weathering, or both. He did find a seasonal change of polarization of light from the "seas," but it did not at all resemble the effects produced by mosses or lichens. He concluded that the surfaces of the "seas" might be covered with *very small opaque mineral grains* or perhaps with "microscopic plants."

This is not the place for an extended discussion of the various possibilities of the nature of Martian plants, if any exist. What appears to be the best discussion of this subject has been given by Strughold (*23*). He concludes that, even under the rigorous conditions on Mars, some low forms of plants comparable to our lichens are possible. On the other hand, there is no proof of the presence of such plants. I consider the widespread belief that vegetation on Mars is "highly probable" or "all but conclusive" as quite unjustified by the available evidence. In any case, the presence or absence of vegetation appears to be rather unimportant one way or the other. It is my opinion that what we see may be, in part, secondarily influenced by vegetation, but primarily the observed surface features have a wholly geologic origin.

One other argument (*24*) that has been presented in favor of vegetation is as follows. Since great duststorms occur frequently on Mars, the yellowish dust of the deserts should soon drift over the dark "seas" and obliterate them, if there were not some process of growth or renewal of whatever it is that produces the dark color. The continued growth of vegetation would admittedly account for the relative permanence of the dark areas. However, the continued deposition of dark dust upon those areas would equally well account for it, provided that we can find a mechanism that would not merely disperse and intimately mix the dust of two colors.

Systematic Patterns on the Surface

Let us now consider the forms of the surface features. In the main, we have only the outlines of areas of contrasting colors. There is little reliable topographic information. It is sometimes stated that the dark areas are depressed relatively to the red "deserts." Probably certain ones of the light areas, such as the round "island" called Hellas, are high plateaus, for frost repeatedly forms there during the night. But, for the surface as a whole, the evidence is inconclusive. However, even the two-dimensional picture may contain some information of value. A geomorphologist could identify Cape Cod as a giant sandspit from an outline map with no contours. He could likewise identify Chesapeake Bay as the drowned valleys of a river and its tributaries. Perhaps the forms of the Martian features contain decipherable clues to their origin. The

relative simplicity of Martian geology (areology) may partly compensate for our lack of detailed topographic data.

The dark "seas" form a girdle south of the equator, but some "bays" project into the northern hemisphere. Figure 1 is an outline map, on Mercator projection. It is based chiefly on the excellent map by Antoniadi (*12*, plates 1–4), which portrays the Martian surface faithfully, so far as it is possible to check the details by photography. Also reproduced here are some of Antoniadi's drawings and some photographs (Figs. 2–7).

The girdle of seas is no random pattern of blotches. Within it there is a peculiar *en echelon* arrangement of parallel bands that trend southeasterly. These include the maria: Sirenum, Cimmerium, Tyrrhenum, and Hadriacum (see Fig. 1). The Sabaeus Sinus also is a ribbonlike marking but trends nearly east and west. Along their northern coasts the banded seas have some branches that terminate in triangular or funnel-shaped estuaries or bays. The final northwestern termination of each band is marked by one or more pointed bays. So long as these tributary or terminal bays are south of the equator, they do not hook back like barbs but may show a slight deviation. However, the bays that cross to the north of the equator do hook back towards the northeast. This tendency of the banded maria to curve at the equator and to end in a hooked-back point is remarkable. The most conspicuous example is the Syrtis Major, the giant of all funnel-shaped bays. Dawes' forked bay, at the west end of Sabaeus Sinus, shows the same behavior; the "barbs" point to the east of north, although they are only a few degrees north of the equator. Approaching from the other direction, the forked bay seems to flow southwestward into the Sabaeus Sinus and immediately curve to the left only a few degrees south of the equator. The Syrtis Major appears to flow southwestward, cross the equator, curve to the left, and continue southeastward into the Tyrrhenum Mare. The Margaritifer Sinus starts at a point, flares as it flows southwestward, crosses the equator, and curves to the left. Any sketch or photograph of these parts of Mars shows an apparent flow pattern from sharp points in the northern hemisphere, across the equator and curving left.

A further point of interest is the orientation of some of the strongest canals in the northern hemisphere, such as Thoth-Nepenthes and Cerberus. These trend from northeast to southwest. Such a systematic orientation was noticed many years ago by Lowell (*15*). My own notice of it was entirely

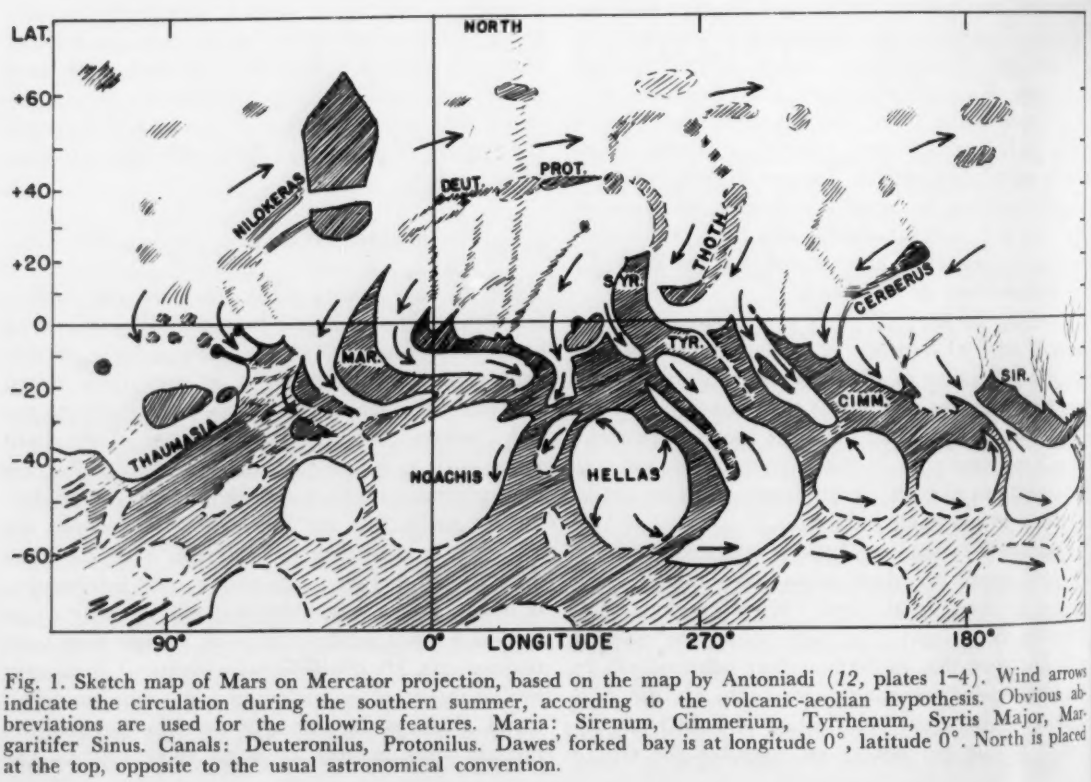
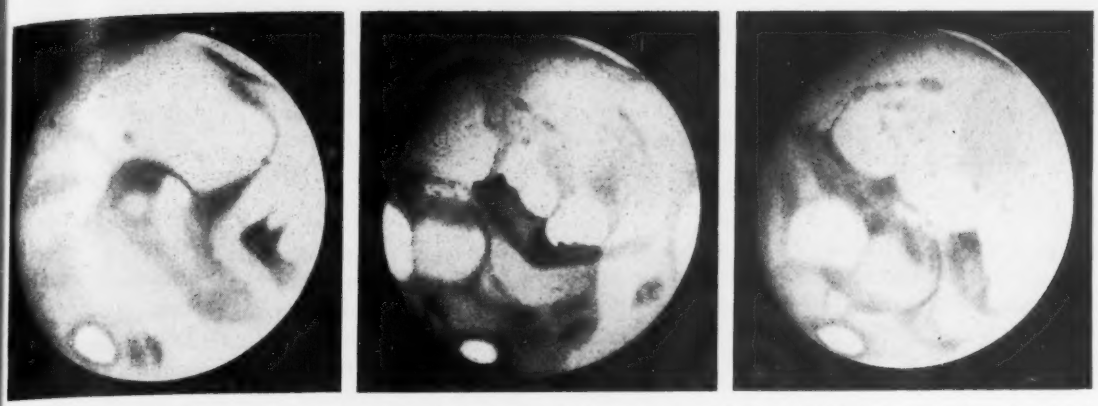
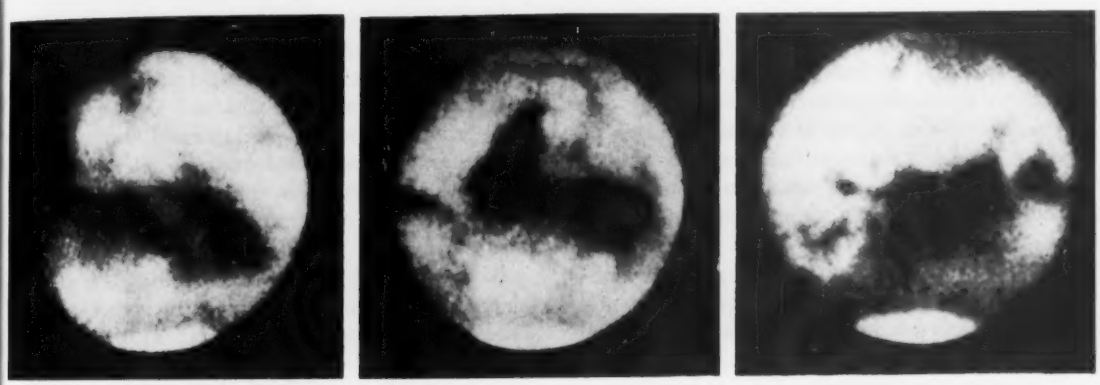


Fig. 1. Sketch map of Mars on Mercator projection, based on the map by Antoniadi (*12*, plates 1–4). Wind arrows indicate the circulation during the southern summer, according to the volcanic-aeolian hypothesis. Obvious abbreviations are used for the following features. Maria: Sirenum, Cimmerium, Tyrrhenum, Syrtis Major, Margaritifer Sinus. Canals: Deuteronilus, Protonilus. Dawes' forked bay is at longitude 0°, latitude 0°. North is placed at the top, opposite to the usual astronomical convention.



Figs. 2–4. Drawings of Mars by E. M. Antoniadi, made during the opposition of 1909. Longitudes of central meridian: Fig. 2 (left), 41° (Margaritifer Sinus right center); Fig. 3 (middle), 160° (Mare Sirenum central); Fig. 4 (right), 197° (Mare Cimmerium). North is upward. [From *Bull. Soc. Astron. France* **24**, 31 (1910)]



Figs. 5–7. Photographs taken by E. C. Slipher of Lowell Observatory, with 27-inch Lamont-Hussey refractor, University of Michigan Southern Station, Bloemfontein, Union of South Africa, during the opposition of 1939. Longitudes of central meridian: Fig. 5 (left), 245° (Maria Tyrrhenum and Cimmerium); Fig. 6 (middle), 275° (Syrtis Major); Fig. 7 (right), 55° (Margaritifer Sinus and Dawes' forked bay at right). North is upward.

independent, during my own observations in 1939 and 1941, and only long afterward was it noticed that Lowell had remarked upon it.

Except for Lowell's notice of canal trends, there has been little mention of systematic orientations of the features. Although the parallel bands of the maria have been shown more or less clearly on all maps of Mars that have been constructed in the past century, there is almost no mention of them in the literature. Antoniadi (*12*, p. 16) makes a very brief, almost casual, statement and drops the subject. This is an amazing circumstance. The pattern exists, it is obvious, yet it provokes almost no comment.

Why was the banding not remarked upon? Perhaps a part of the answer is that the map is a sufficient record of fact. But there must be deeper psychological reasons for the omission. Probably, the observers did not mention the pattern because it conveyed no meaning to them; it raised no questions in their minds. And they failed to see meaning and to think of questions because they were geologically untrained. In a way this was fortunate, for without geologic preconceptions they observed with complete objectivity.

Wind Patterns

The systematic orientations of elongated bands, northeast in the northern hemisphere and southeast in the southern, recall the directions of the terrestrial trade winds (Fig. 8*A*). The correspondence is simply too close to be dismissed as accidental. In view of the practical certainty that aeolian sedimentation is dominant on the surface of Mars, it seems reasonable to adopt the working hypothesis that the banded seas and strong canals represent sand or dust picked up from local areas by the

prevailing winds and deposited to leeward as a fallout pattern.

The curvature of the bands at the equator suggested monsoon winds, but it was some time before the vast scale of the Martian monsoons was recognized. Monsoons are severely limited on the earth, because the great heat capacity of the oceans tends to stabilize the climatic and wind belts. Only where the great continental area of Asia lies north of the equator and the Indian Ocean to the south do the monsoon winds attain strong development. But Mars is *all land*. With the northward and southward migration of the direct solar rays, the low-pressure belt of the heat equator will shift far northward and southward, for the solid surface will heat rapidly. In the southern summer (Fig. 8*B*) the northeast trade winds of the northern hemisphere will continue across the equator, curve to the left in the southern hemisphere, as a result of the Coriolis effect, and continue as northwest monsoon winds to latitudes 30° to 40° south. Mars is nearest the sun during the southern summer, and the winds then will be the strongest of the Martian year, in response to maximum solar heating. In addition, the average color of the southern hemisphere is darker, and this will tend to increase the inequality. In the opposite season (Fig. 8*C*) the winds that cross the equator from south to north will be weaker. The main pattern of deposition will be established in the southern summer, and the weaker winds of the northern summer will produce a less conspicuous pattern.

These expectations are borne out by the main features of the Martian map (Figs. 1 and 8*D*). The monsoon winds explain the flow pattern of the Margaritifer Sinus, Dawes' forked bay, and Syrtis Major-Tyrrhenum Mare. Confirmation of these winds is available in records of moving dust clouds. DeVaucouleurs (*2*, p. 339) has collected the best examples of these. On two occasions great dust clouds were carried along the approximate path outlined by the Syrtis Major and Tyrrhenum Mare, so that the existence of Martian monsoon winds on a vast scale is founded on direct observation.

Volcanoes

The vertices of the pointed bays now claim our attention. If we had not already reasoned out which way the strongest winds blow, these should tell us. Certainly it would be patently absurd to suppose that the dark dust is funneled into the pointed bays. The only rational interpretation is that the vertices point *into* the wind, and that they are tiny but powerful sources of dark matter that is carried from these points, fanning out because of variable wind direction and turbulent mixing. These points must have very special properties. A mere solid rock of different color, weathering into dust in such small areas, could not furnish the material to cover the vast maria. If we restrict ourselves to natural phenomena with which we have experience, these "point sources" seem to have but one probable interpretation: they must be active volcanoes whose ash is carried by the winds and deposited for hundreds of miles to leeward in the pattern that we see.

Many years ago it was recognized that a number of the canals join the seas at pointed bays (*15*, p. 265). Lowell supposed the canals to be waterways and the seas to be irrigated areas. The established dryness of Mars makes any such ideas untenable. But the observed connection of bays and canals finds a full and rational explanation in the volcanic ash hypothesis. When the winds are reversed, during the northern summer, weaker ash trains must extend northward from the same volcanoes whose ash falls of the southern summer made the pointed bays south of the vents. Some of Antoniadi's drawings (for example, Fig. 3) even show

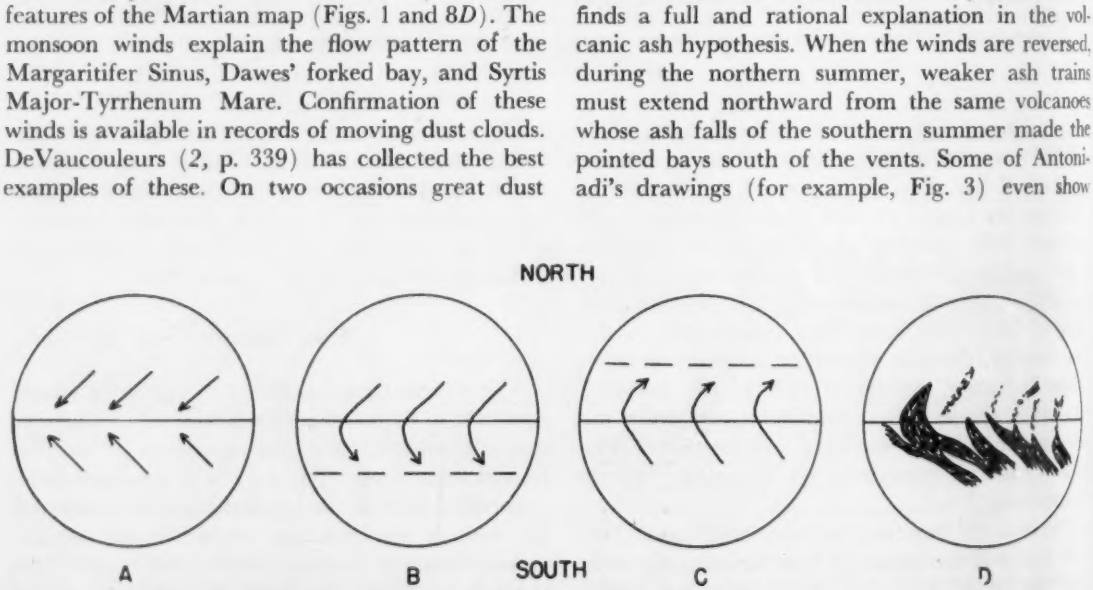


Fig. 8. Trade wind and monsoon patterns. (*A*) Schematic diagram of terrestrial trade winds; (*B*) schematic diagram of monsoon winds of Martian southern summer; (*C*) monsoon winds of Martian northern summer; (*D*) sketch of Martian maria (compare with *B*).

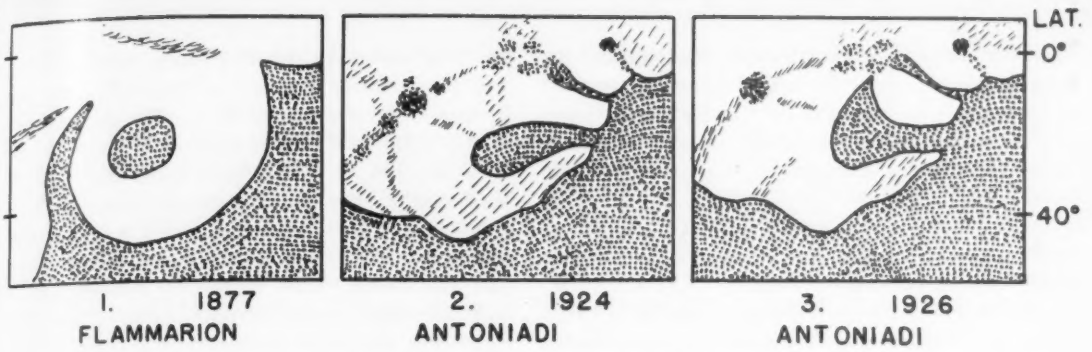


Fig. 9. Changes in the Solis Lacus region of Mars. North at top.

the north-trending canals as fanned-out smudges, in accord with this expectation.

A number of canals in higher northern latitudes have northeasterly or even east-west courses. Either direction appears to be explicable in terms of ash drifts carried by the prevailing westerly winds from volcanoes situated in some of the larger oases. In general, the association of canals with oases finds an explanation in the volcanic ash hypothesis, if we consider each oasis as the site of one or more volcanoes. Seasonal shifting of winds may partly explain the apparent radiation of canals from certain oases. On the other hand, the supposed *connection* of oases by canals, which looks so purposeful and artificial, is most probably a subjective effect.

The volcanic ash hypothesis can contribute some suggestions about the forms of the canals. Different observers have recorded them as fine lines, as broad hazy streaks, or as irregular chains of disconnected spots. What are they actually? Perhaps they are all three, in different parts and at different times. Near a single volcanic vent the fallout pattern may very well be a narrow band; at greater distance it will fan out into a broad hazy streak. Finally, if a considerable time passes before new ash falls, wind action can drift desert dust over parts of the dark ash, breaking the once continuous band into disconnected spots. During the actual progress of an eruption, the long banner of ash trailing to leeward, combined with its shadow on the ground, would possibly be visible as a narrow line. The power of suggestion is great. What observer, on seeing *part* of a canal as an unmistakable narrow line might not conclude that it was narrow throughout its whole course?

Changes Explained by Volcanism

Various aspects of the afore-stated hypothesis have been discussed in several papers and notes published in different places (*20, 25–27*). The ideas of volcanism and aeolian deposition were developed entirely from a study of the forms of the permanent markings on the Martian surface. No consideration was given to changes of any kind. As soon as it was seen that the hypothesis gave a good account of many previously unexplained facts, the problem of changes was examined. Several marked alterations have occurred, and most of these give a satisfactory confirmation of the volcanic-aeolian hypothesis. I have discussed these in detail elsewhere (*27*). We consider here a few of the most conspicuous examples.

In 1783 and 1800, Herschel and Schroeter, respectively, observed a large horn-shaped marking, almost a twin to the Syrtis Major. It had not been present at all in 1719 when Bianchini made a study of the planet. Owing to lack of observations in intervening years, the date of first emergence of the marking is unknown. It was a giant "pointed bay," and it curved as it crossed the equator from north to south, flared out, and joined Mare Cimmerium. At the position of its vertex there is now an oasis, but the horn disappeared many years ago, although there were partial recurrences, notably in 1882. The volcanic-aeolian hypothesis gives a highly satisfactory explanation of the change. At some time between 1719 and 1783 a great eruption occurred at the oasis that occupied the vertex. The fallout pattern of ash formed the great horn, whose curvature was consistent with the Coriolis effect. Later, when activity had ceased, desert dust was deposited over the ash, obliterating the marking, but less violent eruptions caused partial recurrences.

Figure 9 shows the striking changes that have occurred in the Solis Lacus region. Prior to 1877, no conspicuous marking was visible close to the west of Solis Lacus. Certainly, there was no more than a trace of the Phasis "horn" in 1862 when Lockyer (*28*) observed Mars in considerable de-

tail, or in 1864 when Dawes (*29*) constructed his map. But in 1877 the horn was a very prominent feature and was recorded by all observers. Today only a canal of moderate strength marks the place, but a large oasis, Phoenicis Lacus, occupies the position of its vertex. The curvature of the horn agreed with that which the Coriolis effect would require. The explanation is obviously similar to that of the Herschel-Schroeter horn.

Another great change occurred in the Solis Lacus region in 1926. Unlike the previous ones, this is documented by photography. The new marking appeared to fan out southward from a small oasis nearly on the equator, then curve left, and flow across the Solis Lacus (Fig. 9). It has since disappeared. The explanation is the same as that given in the preceding cases. Again the curvature was consistent with the Coriolis effect.

Flammarion (*30*) discussed the Phasis horn of 1877 and suggested that it had been an estuary that had later dried up (he believed that the "seas" were very shallow bodies of water). Today we can accept no such interpretation. Nor does the growth of vegetation help to explain the phenomenon. The ash fallout hypothesis is wholly satisfactory, yet it was developed originally with the idea of explaining the relatively permanent features, and without any consideration of changes. Indeed, the success of volcanism and aeolian deposition in accounting for the changes constitutes a rather rigorous test of the hypothesis.

The amounts of ash required to account for these temporary markings are not excessive (*26*). The area of the Phasis horn was about 250,000 square kilometers. Ash 1 centimeter thick over the whole area (probably an overestimate) would amount to 2.5 cubic kilometers. Comparable and greater amounts have been calculated for terrestrial eruptions (*31*). Regarding the distance to which ash can be transported, there are numerous examples of ash from terrestrial volcanoes having fallen hundreds of miles from its source. Details and specific references are given in a paper of mine (*27*) already mentioned. If the Martian winds can raise huge clouds of dust from the surface and transport it hundreds of miles, they can certainly carry ash just as far, especially if it is raised high in the air by a volcanic updraft!

Condensation Clouds over Volcanoes?

Still another type of evidence has recently come to light. In 1954, Pettit and Richardson (*32*) observed recurrent clouds in a very limited area. A dense cloud close to the position of the strongest one observed in 1954 was photographed by O. H. Truman with the 100-inch telescope in 1941 (*33*). Clouds were photographed in the same area by Wright (*34*) in 1939 and by Ross (*35*) and Wright (*36*) in 1926. If clouds form recurrently over the same region, they must be caused by some mechanism associated with a surface feature. If there were mountain ranges on Mars, we might suppose that the clouds were caused by condensation of moisture from rising currents along a mountain front; but there are no mountain ranges on Mars. Another possibility is condensation of steam that issues from volcanic vents. A striking fact about these clouds is that they have been concentrated in the same area where some of the greatest changes have occurred on the surface. Dense knots of these clouds were close to Phoenicis Lacus, the oasis from which the Phasis "horn" flared out in 1877, and others were not far from the spot from which the temporary marking of 1926 fanned out. Whatever the true cause, this region of Mars will repay careful study.

Unsolved Problem of Seasonal Changes

The hypothesis does not give a convincing explanation of the seasonal changes or of the greenish color of the "seas." The changes may result in part from the drifting of a thin film of desert dust over the dark areas when the winds change. The annual migration of moisture from the shrinking polar cap to the growing one may involve a slight moistening of the surface and a change of tone of some of the dark areas. The seasonal changes remain the strongest single argument for the presence of some form of vegetation. One possibility appears to be that only the regions that are subject to repeated falls of fresh ash and accompanying moisture are able to support vegetation. Thus, the forms of the vegetated areas would be determined by the fallout pattern of the ash, and the seasonal changes could be due to vegetation. The green color may be explained chemically. The weathering of ferromagnesian minerals by carbon dioxide and moisture, in the absence of oxygen, can produce the secondary mineral chlorite, which is greenish. The correct explanation may be something very different.

Objections

Following the publication of the first papers on Martian volcanism, Kuiper (*14*, p. 280) presented several arguments against volcanism. Some of these are based on doubtful analogies of Martian fea-

tures with those of the moon, Mercury, and the four large satellites of Jupiter. If there were conspicuous similarities between lunar and Martian features, this would be a telling argument against the volcanic-aeolian hypothesis. No appreciable resemblance is apparent to me, and since good maps of Mars and the moon exist, the question should not require argument. As for the comparison with Mercury or with Jupiter's satellites, the markings on these objects are so difficult to observe that their very existence was questionable for many years. The very low densities of Jupiter's satellites seem to preclude any close similarity of their composition and structure to that of Mars, and identical origins of the surface features therefore appear improbable.

Kuiper's argument from the dryness of Mars is a point well taken. All terrestrial volcanoes give out large amounts of water vapor. How much of this is recycled ground water is still an open question, but a considerable fraction is probably magmatic water. Many geologists now probably subscribe to the view that the earth's oceans are essentially an accumulation of water brought to the surface by volcanism throughout geologic time (*37*). If volcanism is extensive on Mars, how can there be so little water?

An all-inclusive answer to this question is not likely to be found readily, but it is important to recognize that the question itself is based on an assumed hypothesis and not on a demonstrated fact. There is no reason for serious doubt that magmatic water issues from volcanoes, whether they be on the earth or on Mars. The question that has to be answered is: Where does the water go? Kuiper has pointed out a very real problem, but as an objection to volcanism, his argument appears inconclusive. I have made a few tentative suggestions (*38*) concerning the disappearance of Martian water. Among them is condensation upon ash particles in the air, so that the water becomes incorporated in the surface deposits, where it can combine chemically. Another way in which water can enter the ground is through formation of frost at night, followed by absorption of part of the moisture as the surface is warmed in the early morning. Such water as remains in the air may be mostly in the form of ice spicules. If there are plants of any kind, they may absorb most of the available moisture. All the possibilities of disappearance of water must be examined thoroughly before we can decide whether the dryness of Mars is or is not an argument against volcanism. The escape of water from Mars is still a question on which there is no agreement. Kuiper (*14*, p. 280) considers escape negligible, but Urey (*39*) claims that loss would be appreciable.

Conclusion

The larger Martian markings, as shown on good maps and photographs, can be admitted as demonstrated facts. Since the volcanic-aeolian hypothesis gives such a good account of these facts, as well as of the temporary changes, it is to be preferred to one that leads to a purely negative result or none at all. This is not to claim that volcanism does in fact occur on Mars; direct proof is not available. The volcanoes are still an inference from observation, although if pressed to state what I *believe*, I would say that volcanoes are present. In the present state of the problem, it is possible to imagine that some totally different mechanism may eventually be found to explain the same facts fully as well, but in all geologic processes no inkling of such a mechanism yet appears, and no alternative hypothesis of *any* kind has been proposed to account for the morphology of the Martian markings. The coloring and the changes of color may or may not be due to vegetation, but even if this aspect of the problem were solved, the *forms* of the vegetated areas would still demand explanation (*40*).

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40. Since this article was written, H. C. Urey, in a personal communication, suggests that instead of volcanoes, the "point sources" may be springs, from which moisture is distributed by the winds (just as I suggest volcanic ash is distributed) and thus determines the areas in which vegetation may grow.

New Home for AAAS

Our cover this month shows the main entrance of the new headquarters building of the American Association for the Advancement of Science. The building was completed at the end of May and will be dedicated on 12 October (see page 213). One of the chief novel features of the building is the vertical aluminum louvers, or movable sunshades, which extend the depth of the second and third floors on three sides of the building. The north side does not receive direct sunlight and, hence, has curtained windows instead of louvers.

The louvers, which are set vertically in rows outside the windows, turn during the day by means of an electric motor controlled by an electric clock mechanism. A separate mechanism for each of three sides of the building controls the louvers. The mechanisms are adjusted to move with the sun at different times of the day and different times of the year. They can also be operated manually and set wide open on sunless days and closed completely at night. In addition to regulating the admission of sunlight, the louvers reduce the heat loss in the air-conditioned building. This effects economies in the air-conditioning system and also means that the capacity and, therefore, the initial cost of the air-conditioning system are considerably reduced.

The building, which is trapezoidal in shape, is constructed of reinforced concrete with limestone facing on the two upper stories and a granite-mixture facing on the first floor. The first floor is occupied by the business offices of the Association. The administrative offices and the editorial offices are housed on the second floor. The third floor has been rented to several other scientific organizations: American Society of Photogrammetry, the American Chemical Society for some of its offices, and the National Academy of Sciences on behalf of the American Geophysical Union and the American Geological Institute. The basement is devoted to parking facilities for staff members of the occupants of the building. The top of the elevator shaft and all mechanical devices necessary for the maintenance of the building are housed in the penthouse.

The architectural design was conceived by the firm of Faulkner, Kingsbury, and Stenhouse, and the general contractor was William P. Lipscomb Company.

Interindustry Analysis, New Tool in Economics

JOHN H. CUMBERLAND

Dr. Cumberland, associate professor and assistant director of the Bureau of Business and Economic Research at the University of Maryland, is a specialist in the fields of regional economics and interindustry analysis. He received his training at the University of Maryland and at Harvard University, where he served as teaching fellow in 1950–51. From 1951 to 1953 he was chief of the Critical Materials Section of the Office of the Chief Economist, U.S. Bureau of Mines. He has been at the University of Maryland since 1953.

ECONOMISTS have traditionally studied how individuals, firms, and societies allocate their resources in their attempts to fulfill competing ends or goals. The resources depend on nature and so are limited; the competing goals are as boundless in number and intensity as human ambition. Before discussing how interindustry analysis approaches this problem, I wish to review briefly the economic theories entertained by the mercantilists, the British classical economists, and John Maynard Keynes. For, with the changing conditions of history, economists not only investigate different aspects of the same general problem but seek to correct the deficiencies of earlier schools of thought.

The mercantilists of the 16th and 17th centuries, in analyzing the question of how a national state might grow and prosper, focused attention on the accumulation of the gold and silver reserves necessary to sustain a strong military establishment. In the 18th and 19th centuries the British classical economists concerned themselves with problems of total output, monetary policy, international trade, and the theory of how the individual firm maximizes its profits. In this era of competitive capitalism, it was assumed that the "guiding hand" of pursuit of individual gain under capitalism would automatically result in maximum national output and employment, through the process described by Adam Smith in *The Wealth of Nations* in 1776 (*1*).

Since pursuit of profit by businessmen was assumed automatically to result in maximum employment and production, economists centered their attention on the mechanics of profit maximization and felt little necessity for exploring the conditions of total employment, which would be automatically maximized. The periodic financial panics and market gluts were generally regarded only as temporary departures from full employment equilibrium, requiring no extensive revision of theory.

However, during the extended "Great Depression" of the 1930's, the guiding hand of private interest failed to restore full employment and maximum production. This phenomenon resulted in a large-scale reassessment of economic theory, with searching attention focused on variables of national employment and income which had assumed basic social importance. The new emphasis resulted in a brilliant synthesis by Keynes in *The General Theory of Employment, Interest, and Money* in 1936 (*2*). The term *Keynesian revolution* refers to Keynes' theory, centering attention on the total volume of new investment as the important dynamic variable affecting income and employment. This new approach to economics, now well integrated into the total body of economic theory, added much to the understanding of business cycles and fluctuations.

However, the Keynesian emphasis on large aggregates of consumption, investment, and income sacrifices much of the important detail concerning individual industries. Mobilization problems related to World War II and the Korean outbreak underscored the pressing need of mobilization planners for specific estimates of the manner in which various military and civilian programs would affect the total demand for the output of particular industries and products.

For example, it is not difficult, using historical statistical data, to estimate how much coal, fuel oil, and gas will be required by households from our fuel-producing industries. However, the addition of a large armament program to the civilian output of the economy will vastly expand total fuel requirements. Because of the enormous amount of processing at various stages of operation in our advanced economy, there is no simple, direct relationship between the final demands generated by households and governments and the total output that must be produced by industries of origin. Therefore, for purposes of mobilization planning, it is necessary

to have some technique that is capable of measuring these complex relationships between what is required by households and governments and what must be produced by each industry in order that these consumer demands may be met.

The only analytic technique now available which is capable of handling this problem is the interindustry, or input-output, approach developed by Wassily Leontief of Harvard University and first published in his book, *The Structure of American Economy*, in 1941 (*3*).

The unique promise of the interindustry approach stimulated the interest of the Federal Government during and after World War II. The Korean emergency resulted in a large-scale interagency program, which by 1952 had produced a working model of the U.S. economy divided into 200 separate economic sectors and industries. However, before the results could be tested and applied, the entire program became a casualty of the economy drive after 1953. Research in interindustry analysis is currently being conducted primarily by university and private groups. However, continuing contributions to the unique ability of this apparatus to analyze macroeconomic problems while preserving microeconomic detail, suggest strongly that official interest will revive in the future.

This examination of interindustry analysis is divided into three parts, offering (i) a model of an economic system, which, although simplified, adequately demonstrates the nature of the indirect relationship between final demand and total output; (ii) an account of how such models are applied to actual situations, once the structure of the economy has been determined; and (iii) a discussion of some of the criticisms that have been directed against Leontief's views, with an attempt to answer them.

The Model and Its Uses

Most readers are familiar with problems involving simple simultaneous equations in which the value of the solution for one variable depends on the size of all the other variables. Systems of these equations have extensive applications because of their use in analyzing those perplexing situations in which "everything depends on everything else." In such problems, mathematics is a powerful ally of the human mind, which is severely limited in number of quantitative relationships it can remember and bring to bear on a problem.

An economic model may consist of few or many such equations. One of the most original and imaginative systems of simultaneous equations describing economic problems is the general equilibrium model that was developed toward the end of the last century by Walras (*4*). However, despite its elegance, which is much admired by economists, the difficulty of providing the data and of solving the large number of equations required by the Walrasian system prevented its implementation for operational use. Leontief's contribution has been to develop the powerful general equilibrium concepts into an operational system that can employ existing data to answer pressing current problems, using electronic computers now available.

Walras had set up his equations to answer the question: "Given the distribution of resources, money, and technology, and letting the demand for various products vary, what will be the equilibrium sets of prices and quantities for each product?" Leontief, on the other hand, takes prices as well as technology as given, and develops his set of equations to answer the question: "For a particular set of final demands, what is the size of total output required from each industry?"

This may seem to be a modest enough problem to pose; nevertheless, it is a fundamental one for purposes of mobilization and, as will be shown, it has many other applications. Moreover, it is a difficult problem, for the answers depend not only on the size but also on the varying composition of the final demand that is desired.

In the Leontief system, if we define Y as the final demand variable and X as the total output, then we can say that $Y \times A = X$, where A represents symbolically in highly condensed form the technologic structure of the economy, which is actually a complex set of relationships to be shown later. It is also the keystone of the interindustry system, and its derivation is described in a subsequent section.

Again, speaking of the simplest model in its barest essentials, it is clear that once A has been derived, any number of hypothetical final demands or bills of goods may be quite easily multiplied by A in order to derive estimates of the necessary total outputs required from every industry in order to satisfy this bill of goods. The system simply tells us what must be put into the system in order to get out a given result; hence, the early terminology *input-output*.

Table 1 contains a highly simplified model of the economy, in which all activities have been divided into three processing sectors (agriculture, manufacturing, and service) and one final demand sector (households). The purpose of this division of sectors is simply to indicate that materials and services must first be processed through a number of stages before they may be delivered to final consumers. Reading across each row shows the distribution of sales of the sector named at the left of

the row to each sector named at the top of the column. Therefore, column entries must necessarily indicate all the purchases of the sector named at the top of the column from each sector named at the left of the row. In this table, the distribution of each industry across its row to the three processing industries, plus final demand, equals the total output of that industry. And this total output is equal to total input or purchases for each industry. When the technologic structure is determined, and is expressed as the matrix inverse, then it is the product of the inverse and the final demand that equals the total output.

Row 4, households, shows that every industry purchases labor and management services from households. This results in consumer income with which the households are enabled, as is shown in column 4, to purchase desired combinations of the final output of the sectors to which they contribute their services. This process underlies the concept of the circular flow of the economy, with a flow of production matched by a counterflow of funds forming the income with which to purchase the produced items.

More elaborate models would contain additional rows and columns introducing more elements of realism. For example, government rows and columns would indicate taxes and federal expenditures. Other sectors would show foreign trade, inventories, construction of capital equipment and household savings.

Figure 1 presents the same data as does Table 1, with lines indicating the flows of goods and services between the processing and final demand or household sectors. It will be noted that flows into and out of a sector are equal, although individual flows between sectors are not necessarily equal. The fact that each transaction is both a sale and a purchase, depending on the viewpoints of the parties involved, is indicated by the inflections of the lines. Flows emerge from the right of sectors as sales but each also enters into the left of a sector as a purchase. To simplify the diagram, the processing sectors of agriculture, manufacturing, and service are each represented at three separate places. Household and final demand are shown as a single sector, and are combined for all sales and purchases.

If the real economy were as simple as it is shown in Table 1, then nothing as elaborate as input-output systems would be required. Something as simple as the Keynesian model or a multiple regression approach would suffice. It may be useful to point out that the interindustry model provides the essential detail omitted by the Keynesian model. Although the Keynesian model emphasizes money flows and is useful in analyzing the large aggregates of consumption, investment, and the government sphere, it leaves unanswered a very wide range of problems. For example, during a mobilization period the Keynesian model could be used to tell us, given civilian consumption and private investment, full employment income, the consumption function, and tax rates, how large a budget for military purposes could be expended without exhausting capacity and causing inflation. However, it could not be used to analyze the alternative requirements of steel depending on whether the military program included a large aircraft-carrier program or rather primary emphasis were placed on a stategic air force. The interindustry approach is currently the most comprehensive analytic tool for solving this type of problem.

The interindustry model analyzes the critical movements within the many various sectors which Keynesian analysis lumps together simply as consumption and investment. One might be tempted to answer that nothing as elaborate as an interindustry table is necessary to compute the differential requirements from the steel industry that are dictated by a naval as opposed to an aircraft program, since it is necessary only to add up the alternative amounts of steel embodied in the two different programs.

A moment's reflection should demonstrate that simple attention to the end-product is quite mis-

Table 1. An interindustry flow table in billions of dollars for a highly simplified model of an economic system.

Industry	Processing sectors (A)			+ Final demand (Y)	= Total output (X)
	Agriculture 1	Manufacturing 2	Service 3	Households 4	
1. Agriculture	1	3	2	6	12
2. Manufacturing	4	6	4	10	24
3. Service	2	3	5	8	18
4. Households	5	12	7	6	30
Total	12	24	18	30	84

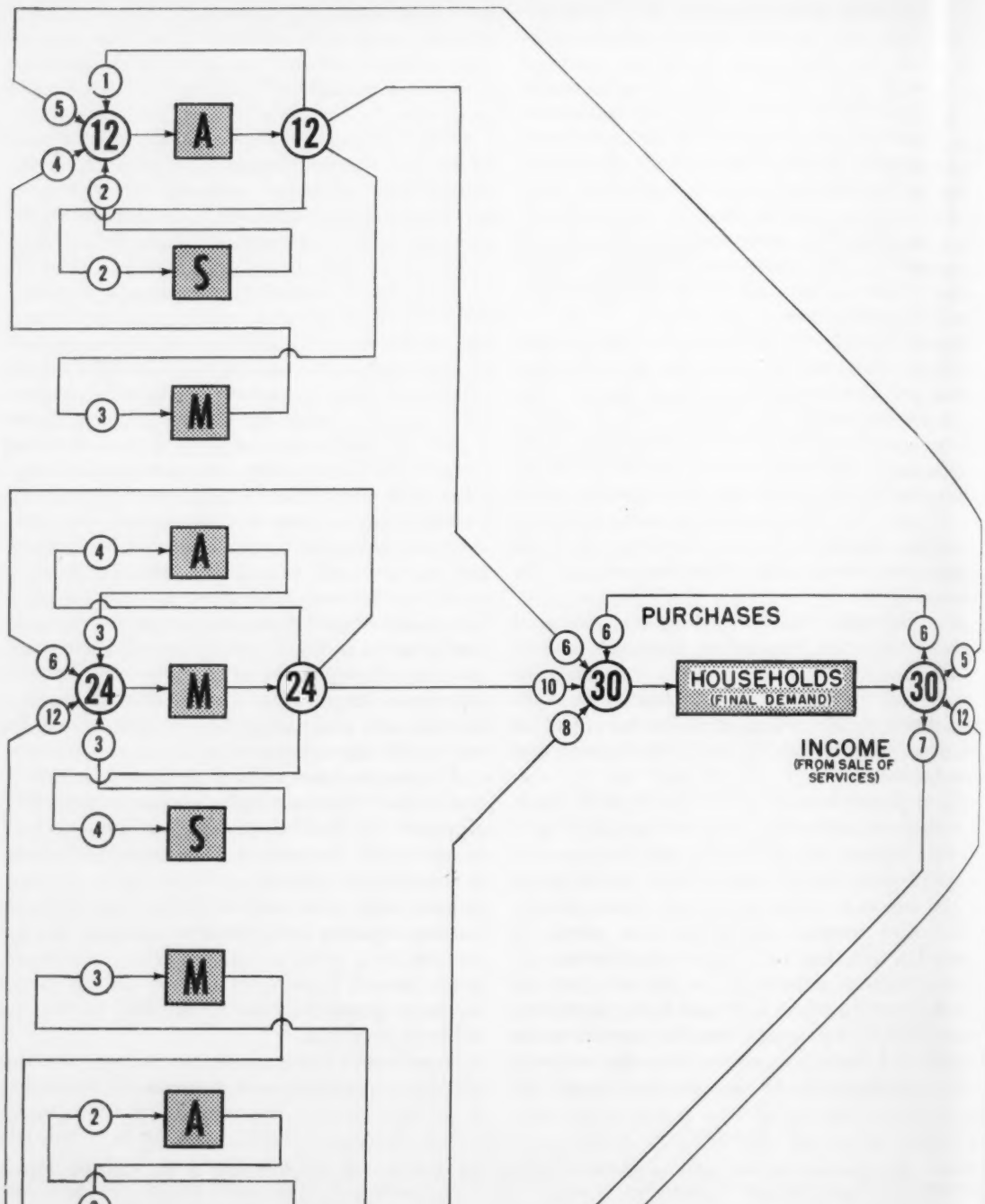

Fig. 1. Diagram in billions of dollars of the same interindustry flow of goods and services as that shown in Table 1. The symbols *A*, *M*, and *S* represent agriculture, manufacturing, and service, respectively.

leading with respect to the total steel required. Production of a ship, for example, requires inputs from hundreds of suppliers, in addition to that from the steel industry, such as machine tools, electronic equipment, heavy machinery, and nonferrous metals. Each of these suppliers must purchase, in order to serve the ship industry, a wide range of inputs, one of which is steel. The suppliers of these secondary inputs, similarly, must purchase materials, including steel. It is true that the magnitude of each of these successive rounds of steel requirements is progressively smaller, but their total is significant.

Another example of the importance of the interindustry flows is federal experience in expanding the chemical industry during World War II. As new needs arose, the War Production Board authorized the construction of plants large enough to meet the needs for new fuels and chemicals stipulated by the armed forces. However, in each instance it was discovered that capacity in the new plants was inadequate to meet total demand, even though ultimate military needs did not exceed expectations. The eventual explanation is easily illustrated in very obvious fashion by the interindustry model. When new chemical plants began operations, they necessarily placed orders with existing plants for raw materials and intermediate products. These supplying firms in turn needed additional supplies of chemicals, and their needs had a resulting impact on the entire chemical economy. It is precisely these secondary and indirect requirements that the mobilization planners had been unable to estimate.

Thus the picture we get of a modern specialized economy is one in which the majority of transactions take place within the processing sectors rather than between the processing sectors and final consumers. This means that for every dollar's worth of consumption by final consumers, a much larger total production of that product must occur. The remainder of this is absorbed within the processing sectors and never reaches the final consumers. Hence, there is need for a model that is capable of estimating the total outputs required from all industries in order that given quantities may be available for consumers.

Considering only one industry—that is to say, reducing our model to a single dimension—we can more simply consider A as a type of reciprocal which shows us, for every industry, the total amount of output required from that industry for each dollar's worth of its production desired by final consumers. For example, if in our base period it is observed that industry 1 produced 4 million units in order to deliver 2 million units to final demand, then our base-year ratio of final demand to gross output is 2/4 or ½. Our A is simply the reciprocal of this, or 2. Then, for any final demand, multiplication by the factor 2 indicates total output required from the industry. The rather lengthy process for deriving A, which is described in a subsequent paragraph, is necessary only because we are dealing with a problem of many variables rather than the one in the simple illustration given here. In actual practice, the total outputs for each industry are actually functions of the required final demand totals from each industry and not just from industry 1. Our A must therefore be a complex function capable of measuring the impact on the total output of every industry by a very wide variety of combinations of final demand.

Much of the complexity of the Leontief system arises from the derivation of A.

Determining the Technologic Structure

The data required in an interindustry table are the distributions of inputs and outputs for each industry included in the system during the base period, as is shown in Table 1. The economy can be divided into as many industries as the breakdown of data and availability of resources permit. This basic table may be designated as a flow table, since it is concerned with transactions on current account and not with stocks or capital equipment. Although the construction of Table 1 is only the initial step in obtaining a workable interindustry model, it has a certain intrinsic value in that it arrays consistently most of the basic economic data of an economy. It is a very useful form of social accounts which are similar in some respects to the businessman's system of double-entry accounting.

The next step is also quite simple and yields us another table. In this step, shown in Table 2, we eliminate from consideration all the columns and rows in the final demand sector, since from now on these will be our variables. We shall now consider only the flows between the industries of the processing sectors. These flows have been converted into ratios by dividing each entry in a column by that column's total. The resulting ratios are called "input coefficients", and the total of each column, including the household coefficients from row 4, must be 1.00. It is clear that each of these is a percentage figure indicating the percentage (in decimal form) of total inputs to that industry supplied by every other industry. More simply, each figure in Table 2 indicates, in dollars, the amount that must be purchased from the industry named at the left of the row in order to produce $1.00 worth of product by the industry named at the top of the column. These

Table 2. The input coefficients for the three industries described in Table 1. The fourth row of input coefficients, referring to households, is usually omitted because these factors are assumed to be variables rather than constants. However, the household coefficients are included here and indicate that the column totals must add to 1.000.

Industry	1	2	3
1	0.083	0.125	0.111
2	0.333	0.250	0.222
3	0.167	0.125	0.278
4	0.417	0.500	0.389
Total	1.000	1.000	1.000

essentially economic coefficients have the merit of being based, in many cases, on engineering relationships that may be used for purposes of verification, or when economic statistics are lacking.

Table 2, a table of input coefficients, is an important step in the derivation of the A table and actually provides all the information needed in A but contains this information in a form that is difficult to use.

Recalling that the desired A is the factor which, when multiplied by final demand, will yield total output requirements, we see that we can use Table 2 for that purpose if we are sufficiently industrious and patient. For example, given a government final demand for $1 billion worth of ships, we can multiply this figure by each input coefficient in the shipbuilding program. The resulting figures will tell us the initially required output from every other industry. Recording these figures, which become a new set of indirect requirements, we can multiply each of them in turn by all the figures in the proper column for each. Clearly, each round of multiplications creates for us an additional burden of multiplications which is increasing at every round by a factor of N, where N equals the number of industries. It is some consolation that the products obtained at each successive round become smaller and smaller. It is less comforting to realize that this overpowering number of computations must be made for every industry. Not only does the burden of the computing problem become intolerable as we deal with larger models, but the simple problem of bookeeping for all of these figures approaches the astronomical if large tables are involved.

Fortunately, however, matrix algebra comes to our aid at this point. The table of input coefficients corresponds to a matrix of coefficients in a system of simultaneous linear equations. It is possible to derive a general solution to this set of linear equations which, once obtained, need never be recomputed, and is independent of any particular set of final demands. The technique used for deriving this general solution, if carried out by hand, is analogous to, and as lengthly as, the process I have described for a particular solution. Fortunately, however, it can be performed on modern electronic computing equipment with great accuracy and with relatively little expenditure of time. The solution to this set of equations will be the long-sought A, or as it is called, the matrix inverse, as is shown in Table 3. When it is multiplied by a set of final demand figures, it immediately gives us in one operation the numbers that would have resulted from the vast number of multiplications just described. In Table 3, each entry shows, per dollar of delivery to final demand by the industry named at the left, total production required directly and indirectly from the industry named at the top. All of the entries on the main diagonal are greater than one, indicating that in order to deliver $1.00 of product from any one industry to final demand, a total production in excess of $1.00 must occur.

In Table 3, the inverse (A) is shown with two examples to illustrate its use. Example 1 shows the inverse multiplied by the original bill of goods (Y) from Table 1. The product should have been precisely identical to the total outputs (X) shown in Table 1. The resulting estimated total outputs diverge slightly from the actual total outputs, because of rounding errors.

Example 2 illustrates a hypothetical bill of goods and the resulting total outputs. The complex effect of final demands on total outputs is well illustrated by comparing examples 1 and 2 in Table 3. Even though final demand for products of industry 1 remains identical in both cases, total output required from industry 1 is much higher in case 2. This is, of course, the result of indirect effects caused by greater final demand for products from industries 2 and 3. Although final demand for industry 2 doubles, total output increases by somewhat less than 100 percent. By contrast, total output required from industry 3 doubles, while final demand has increased only 1¼ times.

These examples demonstrate the significance of secondary effects caused within the processing sectors of the economy. The size of secondary effects is measured, in each example, by comparing the excess of total output for each industry over its delivery to final demand. They further illustrate the danger of assuming any direct relationship between final demand and total outputs, and they underline the necessity for a model capable of measuring the indirect effects (*5*).

The A, or inverse, is a square matrix containing as many rows and columns as would the endogenous sectors of the flow table. The inverse is then

Table 3. Two examples in which the technologic structure, expressed as the transposed matrix inverse, is used to determine total output requirements. Final demand and total output are in billions of dollars.

Industry	Inverse (A)			×	Final demand (Y)	=	Total output (X)
	1	2	3				
			Example 1				
1.	1.2264	0.6570	0.4003		6		11.95
2.	0.2493	1.5398	0.3412		10		23.88
3.	0.2623	0.5677	1.5465		8		18.19
			Example 2				
1.	1.2264	0.6570	0.4003		6		17.07
2.	0.2493	1.5398	0.3412		20		44.96
3.	0.2623	0.5677	1.5465		18		37.06

independent of demand and output in the base period, in general, and can be applied to any theoretical bill of goods. The proper procedure for multiplying this by a bill of goods in order to derive total output is based on the laws of matrix algebra, which, in this case, are quite simple, since both the final demand column and the column of total output can be considered as single vector matrices. In order to obtain the first element in the total output matrix, each element in column 1 of A (*6*) is multiplied in turn by the corresponding element in the final demand column, and the results are summed. Similarly the second element in the total output column is obtained by multiplying in turn each element in column 2 of A by the succeeding elements in the final demand column, and so on for each element in the total output column. Only a single column of final demand figures is necessary to summarize requirements for households, governments, and any other sectors that we wish to specify or consider exogenous. All final demand columns can be added together horizontally so that only one multiplication process is necessary.

Evaluation of Leontief's System

With respect to the basic interindustry model described, we have traced the derivation of a rather elaborate economic model which, having been solved, is then available for general application to a certain range of problems. Economists should be concerned with the question whether the obtainable results justify the necessary expenditure of resources.

Most of the criticisms of the Leontief system are generally well known by now, but they have been recently marshaled in particularly devastating array by Robert Kuenne (*7*). Although Kuenne does not spell out all of his charges, he suggests that the Leontief system depends on the assumptions of inertia and stability. Contending that interdependence and disaggregation are not sufficient to justify the Leontief model, Kuenne objects that Leontief eliminates economic choice and decision-making and considers prices and quantities independently. Kuenne further objects that Leontief ignores the traditional economic concepts of supply and demand, replacing them with the engineering concept of requirements.

Certainly there is a large element of truth in Kuenne's criticisms. The relevance of these points will depend on the types of problems to be analyzed. With regard to inertia and stability, it is true that Leontief's basic model assumes for every industry a linear homogeneous production function which means that all variable costs are strictly proportional to changes in output. Dorfman points out that this formulation also assumes away the critical substitution problem (*8*). Leontief, in answer to these criticisms, has suggested that, over realistic ranges of output, the assumption of linearity is an acceptable approximation of reality. It is probably reasonable to expect that his model will yield realistic answers in cases where the assumed final demand does not diverge sharply in size or composition from that of the base period. As projections are made for consumption patterns whose time period, size, and composition are further removed from those of the base period, it must be assumed that the size of errors will increase.

Leontief further suggests that his model is quite capable of handling nonhomogeneous and even nonlinear production functions. This is quite true, but the operation of such a model would be very costly, and, for large models, would tax the capacity of available computing machinery.

With regard to technologic change, it is true for many industries that not only direction of change but the rate of change over time for particular input coefficients can be predicted. This was done in the 1947 Bureau of Labor Statistics table for the energy and fuel inputs. Rates of increase in fuel efficiencies and substitution between fuels have been rather stable historically. For longer periods of time and for industries where technologic change is not predictable, new basic tables should of course be constructed, perhaps every 5 years as new census data become available.

The flow model which I have described leaves unanswered a wide range of economic problems. However, promising dynamic models of great theoretical interest are now being constructed on an experimental scale. The nature of these models can be described concisely by suggesting that every

cell, in addition to containing flow coefficients, would contain capital coefficients which in general would be based on the familiar concept of the accelerator principle. In other words, final demands which exhaust existing industrial capacity activate capital coefficients, generating demand for additional capital equipment which is fed back into the bill of goods (*9*). Additional theoretical and empirical efforts are being made to introduce spatial elements into economic analysis through the development of regional interindustry tables (*10*).

This cursory survey may suggest some of the limitless possibilities of the interindustry models for examining the problems of growth, development, and economic fluctuations. Only the barest beginnings have been made along these lines.

What of other criticisms? Although it is true that the interindustry system is generally based on the assumption of fixed prices, certain of its formulations can be used to study price changes and have thus been used by Leontief in the second edition of *The Structure of American Economy* (*3*). In general, however, it is quite true that the system is much better adapted to the analysis of the real rather than of the monetary flows within the economy. Kuenne is also correct in suggesting that the interindustry system casts little light on the nature of changes in demand.

The interindustry system is a remarkable instrument for working out in detail the implications of any assumed final demand. The particular set of final demands assumed is thus critical. This presents no particular problem during mobilization periods when military and final demand can be stipulated. For other periods, however, the effectiveness of the Leontief system as a predictive device depends not only on its accuracy as a representation of the structure of the economy but also on the ability of the computer to prognosticate the bill of final demand in the period subject to prediction (*11*).

Kuenne's statement that the concept of supply is ignored is formally correct. However, the interindustry model can be used in conjunction with a set of capacity estimates for each industry which will indicate short-run bottlenecks. The dynamic models described here can be constructed to analyze problems of capacity expansion. Furthermore, it is true that the model can rather easily be changed from the open model that we have been discussing to a closed one that generates or estimates its own final demand, using whatever relationships we wish to specify (*12*). Much of the theoretical appeal of the Leontief system results from the fact that it can handle any type of information given to us by any other theory of economic relationships—for example, any consumption function or supply schedule derived from other analysis can be built in. Thus Kuenne is correct in stating that the open-flow model tells us nothing about demand, but closed models can be constructed to contain any theory of demand given us from other sources.

In conclusion, brief mention should be made of the currently most useful application of the interindustry system, which occurs in the field of feasibility testing of mobilization plans. For example, basic civilian needs plus any military plan to be tested can be added horizontally to form a final demand column. When multiplied by the inverse, the result will be a vector of necessary outputs from each industry. Comparison of these indicated outputs with current capacities in industry immediately reveals whether such a plan is economically feasible. If infeasible, the residuals indicate how much growth is necessary to meet the plan. Thus, in cases where demand can be specified, as under mobilization conditions or planning, the interindustry system offers a powerful tool of analysis.

Another characteristic of the basic Leontief model should be raised at this point. It is a simple equilibrium model which, subject to capacity limitations, is consistent with any level or composition of final demand and is therefore poorly adapted to maximizing. However, by linear programing methods, the model can be made to maximize weighted final demand, subject to any set of capacity figures. This process might be viewed as running the conventional model backwards, and it is one of the better known of the linear programing models. Defense applications are obvious.

At this time it is premature to attempt to estimate whether the simplifying assumptions embodied in contemporary interindustry models are sufficiently unrealistic to vitiate the results, or whether they represent a stroke of genius which at last gives us an acceptably accurate operational model of the entire economy. Critics have questioned whether the interindustry models thus far tested have yielded results superior to those which could have been obtained with much less complex and expensive an analytic apparatus (*13*). However, these empirical tests, admittedly inconclusive, are all based on earlier models more limited in scope and elegance than the 200-order 1947 Bureau of Labor Statistics table, which was never comprehensively evaluated. Even if we ignore the rather promising theoretical and conceptual promise of the interindustry system, the vast amount of resources already expended on the 1947 table appears to justify a greater effort to assess the value of the tool that has been forged.

As for future developments, it must be recognized that economics is a study of human behavior.

As such, its long-run underlying principles rest on patterns of development and learning rather than on mechanical patterns or on the statistical probability of the natural sciences. In the short run, however, there is a strong continuity in the technologic determinants of production and in the interrelationships between these. In developing an operational model based on these structural relationships, and in allowing for changes in them, Leontief has suggested a line of advance in economics which holds definite promise for the future.

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There are men (and I think Priestley was one of them) to whom the satisfaction of throwing down a triumphant fallacy is as great as that which attends the discovery of a new truth, who feel better satisfied with the government of the world, when they have been helping Providence by knocking an imposture on the head; and who care even more for freedom of thought than for mere advance of knowledge. These men are the Carnots who organize victory for truth, and they are, at least, as important as the generals who visibly fight her battles in the field.—T. H. HUXLEY, *"Science and Education," Collected Essays.*

Studies on Deep Mass Culture of Algae in Israel

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WORK on the mass culture of algae has been pursued continuously since preliminary results were described by Evenari, Mayer, and Gottesman (*1*). The reasons then cited for undertaking this work in Israel are as valid today as they were then. As long ago as 1951, it had been decided to approach the problem from a slightly different angle than was then the case in most other centers of research (*1–3*). The approach chosen was that of a deep culture—that is, a culture whose depth was about 1 meter and into which light would be introduced in some way. The method originally planned proved impracticable, and some form of stirring was decided on as the most suitable way of bringing the algae continuously into the light. It is intended to describe some of the results obtained (*4*).

Materials and Methods

The culture unit. The basic unit, in which all the experiments have been carried out, is a concrete tank whose front was built with a perspex or a glass window. This tank was erected in a section of Jerusalem, latitude 31°45′ N and so oriented that the transparent side faces south. Its dimensions were 2 by 1 by 1 meter. The tank was coated with a white polyvinyl chloride paint.

The solution in the tank was agitated mechanically, either by a paddle stirrer (Fig. 1) or, at a later stage, by a stirrer built on the Archimedean screw principle (Fig. 2). The general outlay of the tank, centrifuge, and pump is shown in Fig. 3. The stirrers were operated during daylight only, 10 to 14 hours per day. In the original form of the tank, mirrors were used in order to increase the light on the south and horizontal faces of the wall. These mirrors were adjustable, and they were set according to calculations on the angle of the sun at various times of the year. These settings, as well as the original design of the paddle stirrer, were made by N. Robinson of the solar laboratories, Haifa Institute of Technology. Experience with these mirrors showed that they were not necessary, and they were therefore discarded in the continuation of the experiments. The two types of stirrers and the whole unit are shown in the photographs.

Light conditions are extremely favorable for mass culture work, there being very few cloudy days in Jerusalem. Light intensities are very high. Full data on cloudiness and light intensity in Jerusalem have been given by Ashbel (*5*). Measurements of the light intensities falling on the top surface and the south-facing surface were made (Fig. 4). These were in complete agreement with the measurements of Ashbel. It will be seen that, although in summer the percentage of the light received by the south wall is small, in winter the south face receives almost as much light as the horizontal, top surface.

The temperature of the tank was steady because of the thermal inertia of a large bulk of water enclosed in a concrete tank (Table 1). As a result, for most periods of the year, the tank remained at temperatures favorable for the growth of algae.

Culture medium. The tank, when completely filled, contained some 2000 liters of culture solution. The culture medium is given in Table 2. This medium had a *p*H between 6.0 and 6.5 over long periods of time, the *p*H remaining within these limits for months.

Nutrients were periodically replenished. This was done on a calculated basis according to the mineral composition of the cells and the amount of dry cells, and therefore, minerals, removed.

Carbon dioxide was supplied on a one-dose-a-day basis. Three hundred liters of carbon dioxide were supplied over a period of 1 hour through two Doulton porous candles of dimensions 1 by 8 inches and of porosity *F*11. This amount was found to be entirely adequate. Increasing the dose did not increase yields.

The illuminated area of the tank, when the latter was filled to a depth of 91.5 centimeters, was 3.57 square meters. This figure or corrections to it, according to the depth of liquid, were used for the purpose of making calculations. In some cases, the total amount of light falling on the surfaces has been calculated. This has been done by integrating the areas under the curve for total incident light on a south or horizontal surface and multiplying by the area of the surface. This made it possible to work out the contribution of the two surfaces to the total light received. It does not give absolute amounts of light falling on the surfaces.

Harvesting. Separation of the algae was carried out by the use of a Sharples No. 16A centrifuge. This method is very satisfactory but expensive. Harvesting was done on a batch basis: half the contents of the tank was harvested, and the clear solution was pumped back. Preliminary experiments with other methods of separation were not successful. Neither a milk centrifuge nor settling out of the algae proved to be satisfactory.

The pastelike product obtained from the centrifuge was dried under infrared lamps in order to prevent overheating during drying. Yields are given throughout as grams (dry weight) per square meter of illuminated surface. The relationship between wet and dry weight was fairly constant, the dry weight being about 25 percent of the wet weight.

Table 1. Maximum daily temperature recorded in the tank and in the air in degrees centigrade.

Date (1955)	Maximum air temperature	Maximum water temperature
30 Jan.	14	15
31 Jan.	17	16
1 Feb.	15	17
2 Feb.	18.5	18.5
3 Feb.	19.5	20
4 Feb.	17.5	18.5
5 Apr.	25.5	22
6 Apr.	18	19
7 Apr.	18.5	17
8 Apr.	17	16
9 Apr.	18	14
10 Apr.	17	18
4 Jul.	33.5	27
5 Jul.	33	27
6 Jul.	35	27
7 Jul.	34	27.5
8 Jul.	34	27
9 Jul.	34.5	27

Species. Three species of algae were used: *Chlorella vulgaris,* Hopkins strain, obtained from W. H. Pearsall, University College, London; *Chlorella pyrenoidose* T × 71105, obtained from J. Myers, Austin, Tex.; and *Chlorella ovalis* Butcher, Plymouth 86, obtained from M. Parks, Marine Biological Station, Plymouth, England.

Methods of estimating growth. Three methods were used in order to estimate growth. These were cell count in a hemocytometer, determination of optical density at a wavelength of 600 millimicrons,

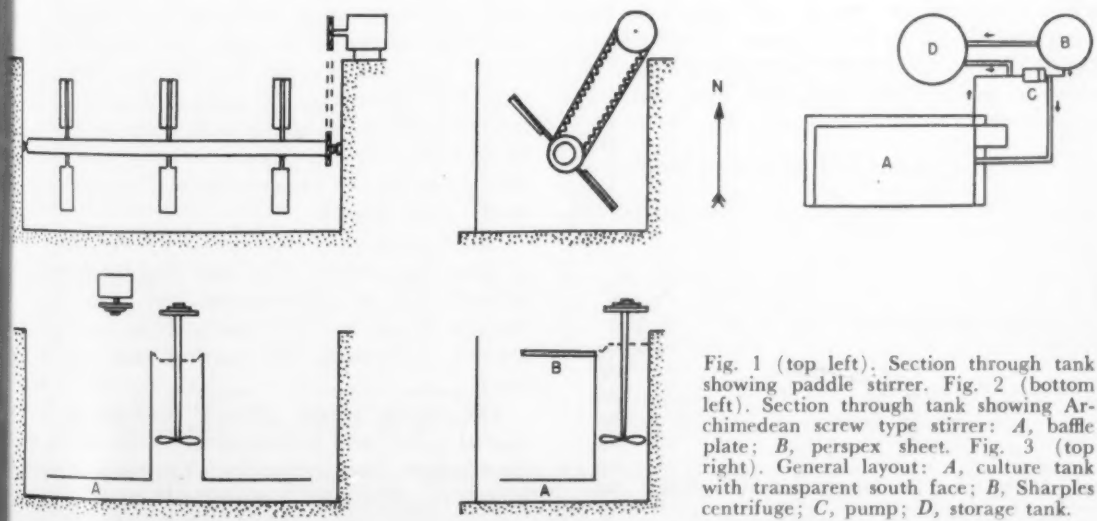
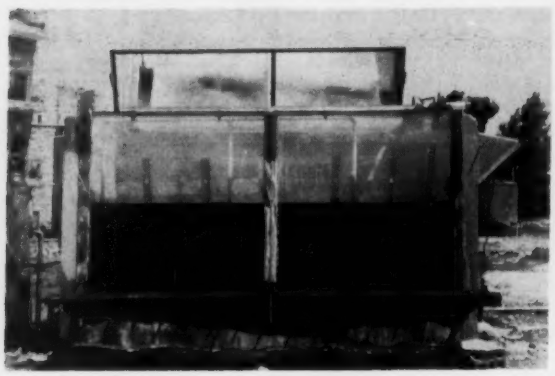


Fig. 1 (top left). Section through tank showing paddle stirrer. Fig. 2 (bottom left). Section through tank showing Archimedean screw type stirrer: *A*, baffle plate; *B*, perspex sheet. Fig. 3 (top right). General layout: *A*, culture tank with transparent south face; *B*, Sharples centrifuge; *C*, pump; *D*, storage tank.



(Left) Tank with paddle stirrer; (right) Tank with Archimedean screw type stirrer.

and harvest yield. The results obtained by these three different methods were poorly correlated (Figs. 5 to 8). Frequently large changes in the number of cells caused only small changes in harvest yield (Fig. 5). Optical density and log cell count did not show a straight-line relationship (Fig. 8). For this reason, neither optical density nor log cell count is a satisfactory method for predicting yields. The actual harvest yield can be determined exactly only by harvesting; it cannot be predicted.

Results

The purpose of this work has been primarily a practical evaluation of yields which can be obtained from mass culture. This discussion of results will therefore be confined to those of practical interest.

Table 3 shows the daily yields for 87 experimental days spread throughout the year. Only runs which continued for several days, usually a week, are given. Yields on isolated days were frequently higher but are entirely unrepresentative. This gives an average, for all species used and for a 1-year period, of 16 grams (dry weight) per square meter of illuminated area, per day. The yields for the different species were as follows: *Chlorella vulgaris,* 16.5 grams (dry weight) per square meter of illuminated area, per day during the months January to May; *Chlorella ovalis,* 18.7 grams in June and July; *Chlorella pyrenoidosa,* 13.29 grams in the period August to September. Presumably *Chlorella vulgaris* would give better yields during the optimal months of the year. The day-to-day variability of the results could not be explained. They did not appear to be correlated with external factors.

These figures assume an equal contribution to the yields of both the top surface and the south-facing wall. Table 4 shows the calculated contribution of the top face toward the total yield. The results are for those months when temperature and light conditions were favorable to good growth.

In Table 5 are given the results of an experiment in which, in a continuous run, both sides were first exposed to the light, and then the south-facing wall was covered. During this time yields were low. Moreover, the light striking the south face at this time is appreciable. Even here it will be seen that the top surface outweighs the south-facing one in the importance of its contribution to the total growth.

In a mass culture, the units of the operation are of great importance. The unit that has been described was an experimental one, and, consequently, it was extremely costly. It is, however, of interest to consider the stirring and harvesting costs.

The paddle stirrer (Fig. 1) was driven by a geared motor and transmission system which was mechanically unsatisfactory. The idle running power required was 420 watts. Under load the

Table 2. Composition of the nutrient solution.

Macroelements	Microelements	Quantity per liter
$(NH_4)_2SO_4$		3 g
$MgSO_4$		1.2 g
KH_2PO_4		2.5 g
Ethylenediamine tetraacetic acid		0.25 g
KOH		0.15 g
	$FeSO_4 \cdot 7\,H_2O$	48 mg
	$MnCl_2 \cdot 4\,H_2O$	27 mg
	$CaCl_2$	11 mg
	$ZnSO_4 \cdot 7\,H_2O$	2.2 mg
	$CuSO_4 \cdot 5\,H_2O$	2.6 mg
	$Co(NO_3)_2 \cdot 6\,H_2O$	6 mg

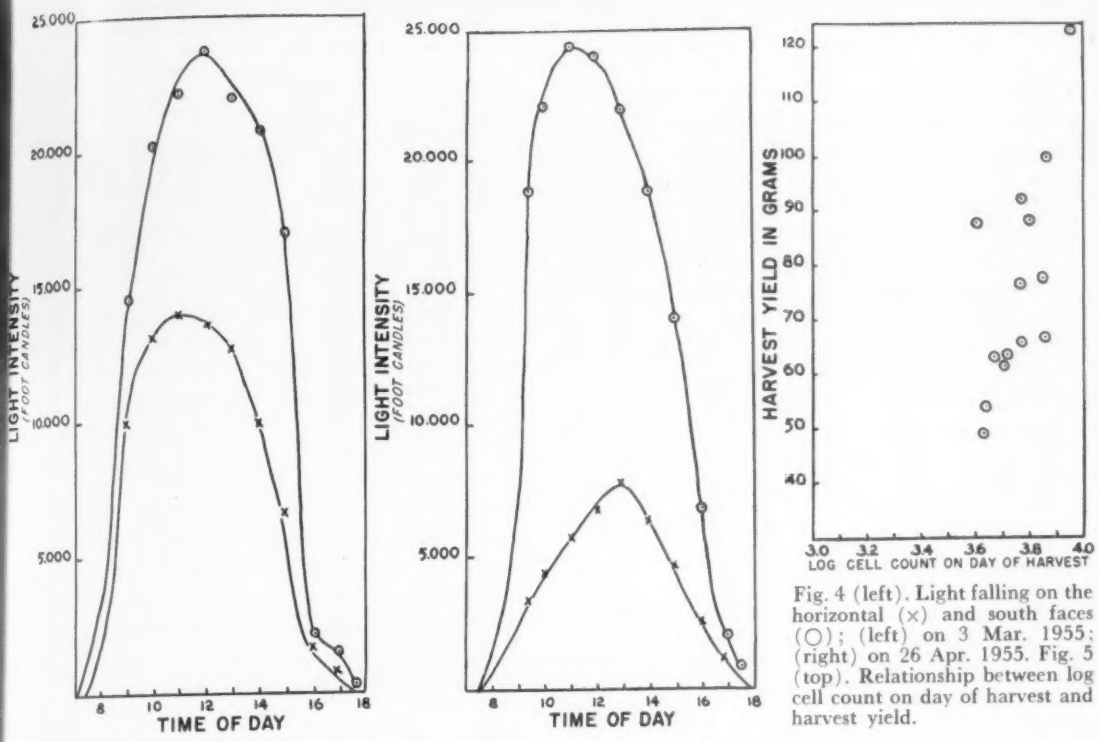


Fig. 4 (left). Light falling on the horizontal (×) and south faces (○); (left) on 3 Mar. 1955; (right) on 26 Apr. 1955. Fig. 5 (top). Relationship between log cell count on day of harvest and harvest yield.

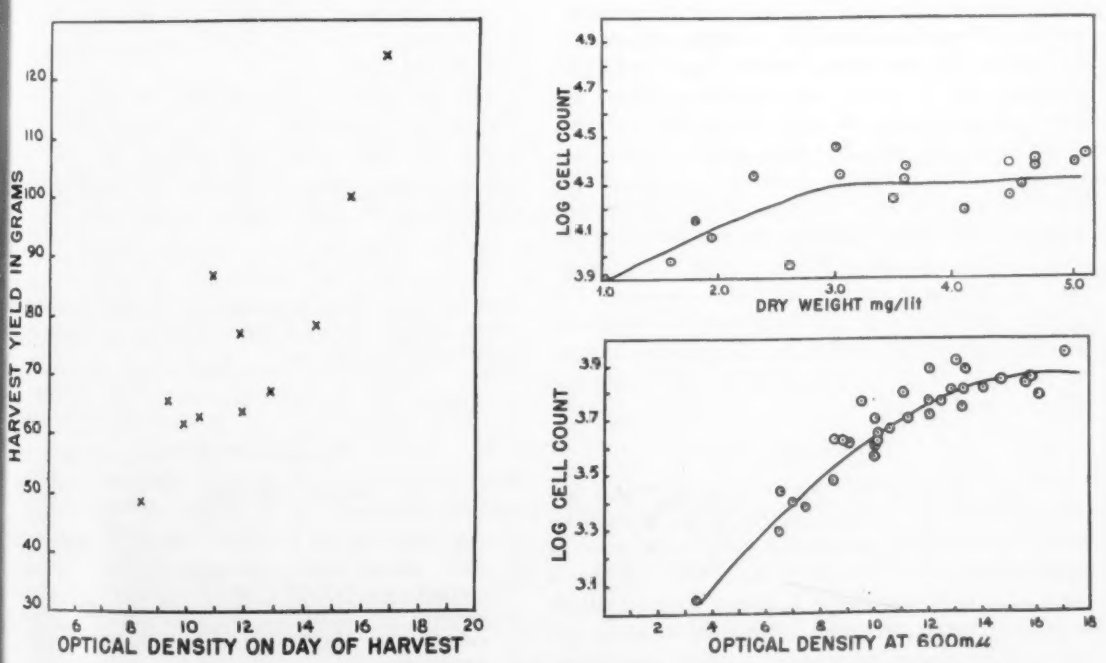

HARVEST YIELD IN GRAMS
120 110 100 90 80 70 60 50 40 30
6 8 10 12 14 16 18 20
OPTICAL DENSITY ON DAY OF HARVEST
LOG CELL COUNT
4.9 4.7 4.5 4.3 4.1 3.9
1.0 2.0 3.0 4.0 5.0
DRY WEIGHT mg/lit
LOG CELL COUNT
3.9 3.7 3.5 3.3 3.1
2 4 6 8 10 12 14 16 18
OPTICAL DENSITY AT 600mμ

Fig. 6 (left). Relationship between optical density on day of harvest and harvest yield. Fig. 7 (top right). Relationship between log cell count and dry weight. Fig. 8 (bottom right). Relationship between log cell count and optical density.

Table 3. Total yield of algae as grams (dry weight) per day, per tank during 1955. The illuminated area of the tank was 3.57 square meters. *Chlorella vulgaris* was grown between 1 January and 20 May; *Chlorella ovalis* between 3 June and 16 July, and *Chlorella pyrenoidosa* T × 71105 between 28 August and 21 November.

Date (1955)	Yield (g)	Date	Yield (g)	Date	Yield (g)	Date	Yield (g)
2 Jan.	104	21 Mar.	124	15 May	73	16 July	68
3 Jan.	72	22 Mar.	92	16 May	71	28 Aug.	50
4 Jan.	61	23 Mar.	87	17 May	70	29 Aug.	33.5
5 Jan.	56.5	24 Mar.	63	18 May	66	2 Sept.	31
6 Jan.	54	30 Mar.	78	19 May	64	8 Sept.	48
8 Jan	48	31 Mar.	77	20 May	44	9 Sept.	40
9 Jan.	42.5	1 Apr.	63.5	3 June	74	28 Sept.	43
11 Jan.	68	2 Apr.	61	5 June	57	29 Sept.	39
12 Jan.	59	15 Apr.	100	6 June	48	30 Sept.	38
13 Jan.	60	16 Apr.	67	7 June	46	1 Oct.	38
14 Jan.	48.5	17 Apr.	62	8 June	50	2 Oct.	38
15 Jan.	54	18 Apr.	66	10 June	49	3 Oct.	41
18 Jan.	80	19 Apr.	63	11 June	40	4 Oct.	39
24 Jan.	85	20 Apr.	88	6 July	112	5 Oct.	43
25 Jan.	67	21 Apr.	54	7 July	96	6 Oct.	35
26 Jan.	64	22 Apr.	50	8 July	73	16 Nov.	84
27 Jan.	55	23 Apr.	57.5	9 July	63	17 Nov.	84
28 Jan.	46	24 Apr.	49	10 July	59	18 Nov.	89
31 Jan.	64	25 Apr.	46	11 July	96.5	19 Nov.	44.5
1 Feb.	45	26 Apr.	46	12 July	70	20 Nov.	47
2 Feb.	55	13 May	56	13 July	62	21 Nov.	42
3 Feb.	47	14 May	88	14 July	70		

power requirement was 630 watts. In other words, 210 watts were needed for adequate agitation.

The Archimedean stirrer (Fig. 2) was operated at two running speeds. The high stirring speed required 128 watts above idle running, which was 302 watts. The low speed, which was found to be adequate and on which our experimental data are based, required only 28 watts above idle running. In other words, only 28 watts were required for agitation. This stirrer, therefore, required 4.6 kilowatts per day with 14 hours of stirring for agitation of 2000 liters and for a yield of 60 grams per day. This calculation is based on our low average yield of 15 grams per square meter, per day. For actual agitation, only 392 watts per day were expended. In contrast to this, centrifugation required an expenditure of 1.5 kilowatts for separating the 60-gram yield.

Discussion

Growth of algal cultures on various scales has been carried out in Israel for the last 5 years. The work outlined here described 2 years' experience in mass culture on a 2000-liter scale. This work shows some extremely promising features. First of all, it is possible to get an appreciable yield of algae per unit of illuminated area under outdoor conditions. These conditions approached those which may be envisaged during factorylike operation of an algal culture plant. The extremely favorable features which may be especially mentioned are septic conditions, economical use of carbon dioxide, and low cost of agitation. Moreover, the ground surface area required was also small, about half of the illuminated area.

The unfavorable features that must be pointed out are two in number: (i) the high cost of separating the algae from the medium and (ii) the cost of construction. The high cost of separating the algae from the medium is the result of two

Table 4. Daily yields as grams (dry weight), average of values obtained during a representative week with three species of *Chlorella*.

Alga	Total yield from tank		Yield (g/m² of illuminated area)	
	Month	(g/day)	Top and side considered equally effective	Top surface only calculated
C. vulgaris	Jan.	69	18.1	21
C. vulgaris	April	71	18.8	26.5
C. vulgaris	May	80	21.2	30
C. pyrenoidosa T × 71105	June	71.5	20	32
C. ovalis	July	51.6	14	25

Table 5. Yield of algae in grams (dry weight) when only the top of the tank is illuminated, in the period September and October 1955.

Yield (g)		Ratio (top/ south surface and top)
South surface and top illuminated	Top only illuminated	
38.2	22.4	.6

factors. The first factor is that the population density—that is, the amount of dry matter per unit value—was very low in these experiments. This means that large volumes of liquid had to be dealt with. The second factor is that the method of centrifugation is in itself an expensive one. The cost of centrifugation was more than the cost of stirring for harvesting the yield of the tank required the operation of the Sharples centrifuge for 1 hour —that is, operating a 2-horsepower motor using 15 kilowatts per hour for a period of 1 hour. The method of separation is, therefore, one of the primary obstacles to converting the prepilot plant experiments in algal culture to pilot and factory levels of operation.

The construction of a tank of the type used is undoubtedly costly. The method of stirring used can be adapted without difficulty to any size of tank. However, a south-facing transparent wall limits the width of the tank and is expensive. It would therefore be necessary, in operating on a large scale, to dispense with the south wall. The figures which have already been quoted give reason to hope that it may be possible to do this without affecting yields adversely.

The depth of the tank was selected arbitrarily. It is extremely doubtful whether light supply is ever a limiting factor for mass cultures in Israel. On the contrary, the problem is more likely to be one of avoiding unduly high light intensities. In practical terms, this means that as far as light is concerned, the depth may be increased. This, however, may cause other complications. It seems probable that there is a certain volume-to-surface relationship which is optimal. At present, efforts are directed at determining this. It already appears possible that the depth of the tank and therefore its volume can be decreased by one-half without adversely affecting yields. This means that the whole unit can be run at a higher population density, which reduces harvesting costs. It can be constructed more cheaply, and it can be operated at lower agitation costs.

One aspect of the work has only been touched upon. It is the one of the algal species or genus most suited for mass culture. *Chlorella* is still the favored organism. It is essential that a systematic screening of algae for mass culture on a large scale, outdoors, must be undertaken. It is not even certain that unicellular algae are the final solution. It may be that filamentous algae are more suitable for mass culture work.

Generally speaking, it may be concluded that the deep mass culture of algae is practically feasible. Economically it is now near the borderline where it can become reasonable, provided that the separation problem can be solved. To what extent the method of mass culture of algae will be applied depends largely on local economic considerations. Climatically speaking, Israel is admirably suited for the purpose.

Summary

A deep unit for the mass culture of algae is described. A cheap method of agitation has been developed. Yields as high as 21 grams (dry weight) per square meter of illuminated area, per day have been obtained. A yearly average for three algal species gave 16-grams. Yields calculated theoretically from the actual yield are as high as 32 grams. The suitability of Israel for mass culture is pointed out. It is considered that the present results bring the mass culture of algae toward the borderline of economic feasibility. Its practical feasibility may be taken as established.

References and Notes

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2. J. L. P. van Oorschot, *Mededel. Landbouwhogeschool Wageningen* **55**, 225 (1955); J. S. Burlew, Ed., *Algal Culture from Laboratory to Pilot Plant* (Carnegie Inst. Wash. Publ. No. 600, Washington, D.C., 1953).
3. R. W. Krauss and W. H. Thomas, *Plant Physiol.* **29**, 205 (1954).
4. Our thanks are due to N. Robinson of the Solar Laboratory, Haifa Institute of Technology, for planning the original culture unit and its stirring mechanism. Our thanks are particularly due to A. Sochachewer for his constant technical advice on planning and construction problems and for suggesting to us the idea of the Archimedean screw stirrer. Lastly, we are greatly indebted to the Ford Foundation who granted the funds which enabled us to carry out this work.
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BOOK REVIEWS

Virginia at Mid-Century. Jean Gottmann. Holt, New York, 1955. vii + 584 pp. Illus. $7.50.

This perceptively written and beautifully manufactured volume establishes a significant bench mark among local-area American geographies. It may well be the forerunner of numbers of small-area geographic analyses of American landscapes executed on a level of professional excellence heretofore exhibited mainly in volumes of greater areal scope and in treatises buried in the pages of scientific journals.

Gottmann addresses himself to the task of analyzing and interpreting the personality of the Old Dominion as revealed through its geographic landscapes. It is his view that the factors that have shaped Virginia into what it is today fall into three groups: natural conditions; "factors resulting from human action"; and the interplay of these two categories as they have evolved through time and in space. To achieve his purpose, Gottmann necessarily must assess evolutions in and contributions from the past, for the past has marked Virginia's land occupance systems and their socioeconomic expressions with traditionalism and conservatism. A historical geography alone, however, would not have served his purpose. Gottmann is compelled to deal with the contemporary scene and the fermenting forces now creating new patterns of urban, industrial, and agrarian occupance in addition to (and frequently at the expense of) what was Virginia only a short time ago.

He has been remarkably successful in this difficult undertaking. His introductory chapters—"The variety of Virginia," "Three and a half centuries of change," and "Regional divisions and modes of life," exhibit a sensitivity to the implications of detailed physical phenomena in the evolvement of an area's culture and economy that is indeed rare. These chapters constitute as high-quality writing in historical geography as I have been privileged to read in recent years. They trace the rise of Virginia's sectionalism based on the subdivisions of natural regions within her borders, the development of systems of land occupance—such as the plantation, the Appalachian hill farm, and others—which later spread throughout much of the South, and the rise of early Virginia as a natural leader of most of the rest of the South. Virginia has a rich history; it would have been easy for Gottmann to have slipped into a recapitulation of matters historical that were irrelevant to the contemporary landscapes—that is, nongeographic. While avoiding this pitfall, the author has adroitly portrayed the Virginian's love for and desire to perpetuate his local past.

The bulk of the volume discusses the uses of Virginia's resources—forests, soils and farmland, and minerals. The foci of these chapters are evolution of utilization systems, modifications of the innate resources through use, and problems presently faced in maintaining permanent, workable connections between the utilizations systems and the resources supporting them. To characterize each of the enormous number of activities which Virginians, utilizing a diverse resource-base, have pursued would have been impossible. And yet there has been a certain similarity in the manner in which Virginians have attacked development of most resources: cautious exploitation, but exploitation nonetheless. Recent industrial developments receive due attention, and their roles in fixing more satisfactory permanent connections between economy and supporting resources are properly noted. Again diversity is a keynote; the variety of industries, now flourishing in a state that many persist in considering limited in its range of urban employment, is symptomatic of Virginia's recent economic progress and of that in much of the South as well.

If Gottmann had been discussing any other state along the Atlantic seaboard, much of his talent for interpreting the significance of location as a factor in cultural and economic change would have been wasted. He repeatedly underscores the view that Virginia's "middleman" location between Northeast and Southeast not only is responsible for the current rapid infusion of new traits from either side, but also enables Virginia to serve as a disseminator of various cultural features to surrounding regions. To an important extent, the early leadership of Virginia in American affairs stems from this quality of location plus the advantage of early settlement.

Today the "middleman" location still plays its role; the development of good roads, mass communication media, and improved schools in the Old Dominion in recent years precipitated the breakdown of the small, self-contained "compartments of space" which were the areal fundaments of the strict social and economic hierarchies characteristic of most of the Southeast until recently. Contact with outsiders has placed new ideas and new social values among the old. Outmigration, farm mechanization, industrialization, and urban developments have depended to a large degree on the disintegration of the old, ruralized, small "space compartments" and their reorganization into an areal fun-

dament that stretches far beyond the borders of the state. Virginia has been a leader among southern states in this "breaking of partitions of space" and, thanks to her middleman location, these processes have spread from Virginia southward and westward into many sectors heretofore noted for the dominance of rural isolationism.

Some might wish for greater quantification of data than Gottmann has provided. Gottmann was trained in the French school of geographers, which has demanded of its practitioners field observation and analysis, reobservation and reanalysis, followed by lucid expository writing intricately matched to field observations. A great deal of quantification might well have produced no more than the existing product and probably would have sacrificed subtleties in expression which lend distinctiveness to the whole volume.

The numerous photographs are outstanding. Noteworthy is a group of vertical aerial photographs, selected with discrimination, which convey more of the areal arrangements of Virginia landscapes than could scores of pages of text. If the volume has a weakness worth noting it is in its cartography. While superior bits of draftsmanship, the numerous maps are mainly exhibits of distributions based on county-unit data. Had they been designed as analytic devices to assess spatial organization of areas, they would have been more nearly integral to the text instead of illustrations of it. Unfortunately, line symbols were selected to represent areal differences in densities of distributions in many instances in which current practices call for dot and screen symbols, or for isolines representing distributions of proportionate values.

The Old Dominion Foundation, which financed this undertaking, deserves special commendation. It is not too much to hope that other scholars and other civic leaders with resources to support such enterprises will join hands to produce the same understanding of areas and peoples elsewhere in our land as we now have in *Virginia At Mid-Century.*

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Carl Friedrich Gauss: Titan of Science. A study of his life and work. G. Waldo Dunnington. Exposition Press, New York, 1955. 479 pp. + plates. $6.

Gauss (1777–1855) is generally considered one of the three greatest mathematicians of all time. His work marked the beginning of a new epoch in mathematics; the high standard of rigor and completeness set by him decisively shaped the subsequent development of this discipline for generations. No narrow specialist was he. As a student he had difficulty in deciding between a career in philology and one in science and mathematics. His decision in favor of the latter was of incalculable importance in the history of these subjects.

In each of the fields of number theory, geometry, analysis, and probability, Gauss did enough to immortalize his name. He was by profession an astronomer; his position at the University of Göttingen was director of the observatory. His first wide-spread recognition was for spectacular work in calculating orbits of planets; his methods revolutionized the solution of this very difficult problem. He was also a practical geodesist; he conducted a geodetic survey of the Kingdom of Hanover and invented the heliotrope. In the field of physics, he made significant and far-reaching contributions to the theory of magnetism and electricity and jointly with his colleague Weber invented the telegraph.

Dunnington presents in his biography a full and absorbing account of this "titan of science," his childhood and education, his family, professional associates, and friends, his discoveries, inventions, and honors, his attitude toward mathematics and science. There emerges the portrait of a genius, born in humble circumstances, exhibiting his great creative powers at an early age and retaining them to the end of his life, conservative in his social and political views, kind but somewhat stern in his relations with his children, subject to the sorrows that may come to any man, and—above all—a man who set for himself a standard of perfection in his work concerning which he would not compromise.

Where possible, the words of Gauss himself, as expressed in his correspondence, are wisely allowed to express his views, and his scientific contemporaries are quoted freely. One acquires a feeling for the environment and times in which Gauss lived and worked.

A valuable feature of the book is a set of appendixes that include estimates by eminent mathematicians of Gauss' contributions, a list of his honors, his will, an extensive genealogy, his views, his publications, and a general bibliography.

Much of Gauss' time and energies was devoted to making measurements and calculations in practical astronomy and geodesy. Regret has been expressed that such tasks, which could have been performed by lesser men, interfered with researches in which only his great imaginative powers could succeed. Gauss himself said, "All the measurements in the world are not worth one theorem by which the science of eternal truths is genuinely advanced." But, like Newton, he derived inspiration for his

mathematical ideas from the natural world, and he believed that much of the significance of mathematics lies in its application to natural science.

Gauss says explicitly, "I have a true aversion to teaching." The small number of mathematics courses he taught bears testimony to the sincerity of this statement. In proportion to the tremendous amount of mathematical research he accomplished, the number of mathematical papers he published is not large, but, in the words of the mathematician Kummer, "They are masterpieces which bear in themselves that character of classicity which guarantees that they will be used and studied with diligence for all times."

A full-scale biography of Gauss has been long overdue. G. W. Dunnington has rendered a great service to mathematics and science in presenting in such a complete and interesting manner the life and works of the man who contributed so profoundly to so many fields of knowledge.

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Free Skin Grafting in Patients with Extensive Skin Defects. B. A. Petrov. State Publishing House of Medical Literature, Moscow, U.S.S.R., 1950. 116 pp. Illus. (In Russian)

In 1869, J. L. Reverdin [*Greffe epidermique, Bull. Soc. imp. de chir. de Paris* **10,** 493 (1869)], a French surgeon, achieved the first successful autogenous skin transplantation in man. Although only a few small pieces of skin were grafted, this operation marked the beginning of a brilliant chapter in the development of reconstructive and reparative surgery. B. A. Petrov's very elegant and profusely illustrated monograph represents a fitting memorial to Reverdin's skill and vision. Most of the patients, according to the author, could not have survived without the aid of the extensive skin grafting performed by Petrov and his associates.

The skin transplantation technique originated by Reverdin involved the utilization of the entire skin thickness. It was further developed by a Russian surgeon, S. M. Yanovich-Chaĭnskiĭ, in 1871 ["Treatment of ulcerations by transplantation of skin pieces," *Meditsinskiĭ Vestnik,* No. 2, 9; No. 4, 25 (1871)] and by J. S. Davis in 1914 ["The use of small deep skin grafts." *J. Am. Med. Assoc.* **63,** 985 (1914)]. P. Pyasetskiĭ in 1870 ["On the transplantation of skin as a method of treating ulcerations," *Moskovskaya Meditsinskaya Gazeta,* No. 49, 415; No. 50, 421 (1870)] reported on the successful transplantation of small pieces of skin into invaginations dug into the granulated regions. This method prevented the shifting of the grafted skin pieces. It was utilized on a larger scale by W. Braun in 1920 ["Zur Technik der Hautpfropfung," *Zentr. Chir.* **47,** 1555 (1920)].

K. Thiersch in 1886 ["Ueber Hautverpflanzung," *Verhandl. deut. Ges. Chir.* **15,** 17 (1886)] proposed the transplantation of large but thin skin sections. According to Petrov, neither the thin skin sections nor the entire skin pieces represent satisfactory materials for grafting. He utilized chiefly the method developed by Padgett (1939), which involved the transplantation of skin sections equal to about three-fourths of the skin thickness. For removing the skin from the donor areas, he used a modification of Padgett's dermatome [M. V. Kolokol'tsev, "Free Grafting of Skin by Means of a Dermatome," *Proc. All-Russian Conference on Problems of Traumatization* (Leningrad, 1948), pp. 127–128].

Petrov carefully details his experiences with extensive autogenous skin grafting in 70 cases of widespread skin losses, chiefly those resulting from third-degree burns. The work was done at the Moscow Sklifosovskiĭ Institute, famous for its part in the development of the blood bank (by S. Yudin *et al.*), during World War II. Petrov occasionally resorted to homogenous skin grafting, utilizing in some cases cadaver donors. Although these grafts at best survived for no more than about 1 to 2 months, the procedure was nevertheless useful as a temporary measure in patients with unusually large skin losses, severe anemia and weakness.

Of the 70 cases of free skin grafting, seven were complete failures. In three patients only 30 to 40 percent of the skin grafts survived. The author ascribes the failures to two major causes: insufficient preparation of the patients (too low hemoglobin, and so forth) and beginning the transplantations more than a month after the trauma. The latter cases arrived at the Sklifosovskiĭ Institute 76 to 172 days after the original injury. After such periods many of the organs of these patients showed irreversible pathological changes. Petrov urges the initiation of skin grafting at the earliest possible moment after the trauma.

The book includes an extensive bibliography, many case-histories with black-and-white and color photographs, as well as many technical details, and a discussion of basic physiologic problems.

SAMUEL A. CORSON

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University of Arkansas School of Medicine

Microbiology. Florence C. Kelly and K. Eileen Hite. Appelton-Century-Crofts, New York, ed. 2, 1955. xi + 615 pp. Illus. $7.50.

In the preface, the authors state that "this textbook is intended for the student who will require a comprehensive knowledge in the fields of general and pathogenic microbiology." An attempt was made, apparently, to supply a work that could be used alike by general college students, nurses, and medical students, but an unhappy compromise in exposition has resulted.

The book is poorly organized, frequently repetitious, usually verbose in the explanation of minor or unimportant details, and almost invariably telegraphic in areas warranting more complete discussion. It is, moreover, an unhandy combination of the old and new; basically, the text has a strong flavor of microbiological antiquity, and material of more recent date has been heaped upon it in jackstraw fashion.

The specific points requiring critical comment are so numerous that only selected examples may be mentioned. Nearly one-third of the book is devoted to taxonomy in the classical manner and is replete with technical descriptions, often of outmoded procedures and apparatus. The section on bacterial physiology is woefully short. Antigens, antibodies, and the phenomena of immunity are handled primarily from the standpoint of the era antedating the introduction of quantitative immunochemistry and Ehrlich's obsolete side-chain hypothesis is presented once again, complete with diagrams.

Reactions involving complement are discussed, but the fact that it consists of several components is not even mentioned. Modern chemotherapeutic and antibiotic agents are listed by name only; in not a single instance is the reader given any information concerning the chemical structure of these compounds. Uncritical and empirical statements are made about the use of antibiotics in the management of infectious diseases, and the subject of antibiotic synergism and antagonism receives similar treatment. Several of the older sulfonamide drugs—for example, sulfapyridine and sulfathiazole—are referred to in such a way that the novice may be led erroneously into the belief that they are still used clinically.

Serum therapy is discussed at some length in connection with the treatment of pneumococcal and meningococcal infections and receives more space than does chemotherapy of these infections. Immunity in syphilis is still described as infection immunity. Wassermann antibody is "reagin," positive standard serologic tests in diseases other than syphilis and yaws are "false positive," and no reference is made to specific treponemal antibodies or to the treponemal immobilization test.

The entire section on viral diseases covers only 25 pages and is virtually worthless; it is interesting to note, in comparison, that 27 pages are devoted to the combined chapters on bacteriostasis and disinfection, and surgical and medical asepsis. The illustrations are unexceptional, since only a few are original and many have been copied from other textbooks.

HARRY M. ROSE
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The Principles of Mechanics. Presented in a new form. Heinrich Hertz. Translated by D. E. Jones and J. T. Walley. New introduction by Robert S. Cohen. Dover, New York, 1956. 274 pp. $1.75, paper. (Unaltered republication of ed. 1.)

Acclaimed by the entire scientific community for his experimental confirmation in 1888 of Maxwell's hypothesis of electromagnetic waves, Heinrich Hertz is known also to another, if smaller, audience for his extended analysis, published 6 years later, of the concept of force. The need in the latter part of the 19th century for Hertz' *The Principles of Mechanics* may be seen, among other places, in the once much consulted *Treatise on Natural Philosophy* by Lord Kelvin (then William Thomson) and P. G. Tait. The treatment of kinematics by Kelvin and Tait is straightforward, if lengthy, but their development of dynamics shows thinking that is often anthropomorphic. The reader finds, for example, that "Matter has an innate power of resisting external influences, so that every body, as far as it can, remains at rest or moves uniformly in a straight line." Also, as Hertz himself notes, Kelvin and Tait still use Newton's definition of mass as the product of volume and density.

Hertz' concern about the foundations of mechanics goes deeper than an impatience with loose language and poor definitions. In the introduction to *The Principles of Mechanics* he offers a set of three criteria for judging the relative merits of competing physical theories. The set states that (i) a theory should be logically permissible, that is, arranged deductively, with the less general propositions following logically from the first principles, once the requisite special assumptions are introduced; (ii) a theory should be correct, that is, agree with experiment; and (iii) a theory should be appropriate, that is, the notation, definitions, and

other arbitrary elements should, in a sense that the introduction makes clear, be distinct and simple. The body of the book is the presentation of the principles of mechanics in a new form. Given the proposed set of criteria, the new form, so Hertz argues, is superior to the more conventional forms, employing as they must the concept of force or, as in Hamilton's principle, that of energy. The theoretical primitive concepts of the new form are space, time, and mass (more accurately, material particles). To provide the empirical realization of this form that constitutes experimental mechanics, each primitive concept has its own law of transformation or, as we should say today, its operational definition. The concepts of force and energy are defined in terms of the primitives and appear only as aids to calculation.

Specifically, the set of axioms of the new form of mechanics consists of a single fundamental law of natural motion, into which is condensed Newton's law of inertia and a minimal principal that Gauss formulated in 1829. The law asserts that all material systems, within the limits of certain rigid connections thought to obtain among their parts, approach as nearly as possible to motion in a straight line with a constant velocity. In addition to the fundamental law, there is a fundamental hypothesis which admits that some of these masses and connections may be concealed. Not all masses are directly presented to the senses, although in other respects all masses are alike. What formerly went under the name of force is now interpreted to result from the motion of observed masses and, when required, concealed masses. Hertz has reduced dynamics to kinematics.

The introduction and the book proper are of more than historical interest. Today, anyone with questions to ask about the foundations of classical mechanics will find answers here. However, two qualifications of this recommendation are necessary. To modern ears, *The Principles of Mechanics,* despite the emphasis on formal reasoning, still echoes those attempts during the 19th century, especially in England, to provide a dynamical explanation of forces observed to act at a distance. Hertz does offer his concealed masses as a physical hypothesis that may someday be confirmed. The other qualification is that, in recent years, attempts at the reconstruction of the foundations of classical and relativistic mechanics, and other scientific disciplines as well, have taken somewhat different directions. One approach, for example, seeks to provide a set-theoretical definition for the concepts of the science it investigates.

In reprinting the translation of Hertz' work, Dover Publications continue their service of making readily available the classics of the philosophy of science. Unfortunately, as sometimes happens in this series, there are oversights. In the present case, the publishers have neglected to number the pages of the opening part of the book. This part includes a new introductory essay by Robert S. Cohen (which, at one point, by another oversight, confuses potential with kinetic energy), the editor's and translators' notes of 1899, a moving preface written shortly after Hertz' untimely death by his great teacher Helmholtz, and the author's preface.

JOSEPH TURNER
American Association for the Advancement of Science

Birthplace of the Winds. Ted Bank II. Crowell, New York, 1956. 274 pp. Illus. $4.50.

Ted Bank has written an exciting adventure story of his first botanical and archeological collecting trip to the Aleutian Islands. It is a very personal account, reflecting the author's interests and sympathies, but is also marred by the expression of his antipathies and professional jealousies. The beauty of the islands, the dangers faced, and the obstacles overcome are described with spirit, and the book is well illustrated with 16 plates and numerous sketches and maps. Because the author and his companion were at the time students at the University of Michigan, the story should have a special appeal to young people.

The author gives us an excellent picture of the dignity of the Aleuts and of the pathetic conditions under which these once prosperous people are now dying off. It is easy to see why he and others have found the Aleuts so charming as individuals and their aboriginal culture so interesting.

In discussing his search for ancient Aleut burial places and village sites, Bank makes clear the need for careful excavation accompanied by adequate record keeping. The important sites of the Aleutian Islands have unfortunately suffered extensively through the building of Army airfields and other installations and through the depredations of curio hunters. However, I would add that Federal authorization to excavate on public lands, as required by the Antiquities Act of 1906, is not simply another instance of governmental red tape, but is an attempt to protect archeological and historical sites from destruction by those who have not been specifically trained in archeology.

FREDERICA DE LAGUNA
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Guided Missiles in War and Peace. Nels A. Parson, Jr. Harvard University Press, Cambridge, Mass., 1956. 161 pp. Illus. $3.50.

Guided Missiles in War and Peace is a simply written book explaining the nature and problems of guided missiles. It develops the problems of propulsion and of guidance and discusses in more detail the operational use of missiles in land and sea warfare. A short section deals with the peaceful uses of missiles and refers briefly to aspects of space travel. The value of a book of this type is quite apparent. It is nontechnical enough to be completely understandable to the layman and at the same time is quite factual. One wishes that there had been further development of the peaceful applications of missiles, for example, the applications to upper atmosphere research and astrophysical research and the recent developments that have led to the adoption of a satellite project in the United States. The book contains many good photographs, but there are few references on the technical side of the problem and, unfortunately, no index.

S. F. SINGER
Physics Department, University of Maryland

The Piltdown Forgery. J. S. Weiner. Oxford University Press London, New York, and Toronto, 1955. xii + 214 pp. Illus. $3.50.

Piltdown Man, the so-called "Eoanthropus dawsoni," was generally (although not universally) accepted for approximately 40 years as an important fossil link in the chain of human evolution. His exposure as a fraud—as a synthetic monster compounded of the brain-case of a relatively recent, modern type of man and the doctored lower jaw of a recent anthropoid ape—rocked paleontological and anthropological circles in 1953.

The story of the hoax, as now revealed, is presented in detail by J. S. Weiner, the Oxford anthropologist who first suspected that the Sussex "fossil" was a concatenated chimera, and at whose insistence the anatomical and chemical investigations were undertaken which proved beyond doubt that the "earliest Englishman" was a diabolically clever forgery. The discovery of the skull, the ensuing scientific controversies, the problems that acceptance of the skull as an actual fossil entailed, and the events leading to the exposure of its true nature, are set forth by Weiner in an absorbing manner. No detective story of fiction could be more engrossing or more challenging. The personalities involved receive considerable attention—not only the principal characters but also others not at all well known generally, such as Lewis Abbott, "the fiery, bombastic, inspiring, and weird" jeweler and amateur geologist; Harry Morris, bank clerk and amateur archeologist; Maj. R. A. Marriott, governor of Lewes jail, another amateur archeologist; and Capt. Guy St. Barbe. Without this information, any real attempt to fathom this amazing forgery and the factors involved would be well nigh impossible.

Who was the perpetrator of the fraud? The evidence, as assembled by the author of this book, clearly points its finger at Charles Dawson, the lawyer-antiquarian of Lewes, Sussex, who discovered Piltdown Man. But Weiner is generous and returns the verdict "not proved," thus giving Dawson quite the benefit of the doubt. He even suggests the improbable though not impossible idea that Dawson "might . . . have been implicated in a 'joke,' perhaps not even his own, which went too far." In this connection, it may be noted that whereas the majority of interested scientists—although by no means all of them—accepted the brain-case and the jaw as belonging to one and the same individual and hence as a natural fossil, a number of Dawson's local acquaintances, such as Marriott, Morris, Pollard, and St. Barbe, regarded Piltdown Man as a deliberate fraud.

In view of its general excellence, one hesitates to note that this book exhibits signs of having been produced somewhat hastily; for it contains a number of errors of proofreading as well as a few of fact. Fortunately, none of these are really serious, and they do not impair the usefulness of the book.

P. S. Weiner notes that Sir Arthur Conan Doyle visited the excavations at Piltdown several times in 1912, prior to the public announcement of the discovery of the skull. At that time, the creator of Sherlock Holmes was engaged in writing his brilliant paleontological extravaganza, *The Lost World*. A pity that he had turned aside from recording the feats of Holmes! For the immortal Sherlock might then have whispered to him, respecting the incongruities between brain-case and jaw, what he whispered to Weiner 41 years later (see p. 35): that "when you have eliminated the impossible, whatever remains, *however improbable*, must be the truth" (originally spoken by Holmes to Watson in another connection, in *The Sign of Four*). What then, if Holmes' warning had been relayed by Doyle to Smith Woodward, the paleontologist who reconstructed Piltdown Man? Would it have prevented Piltdown Man from being officially born into polite scientific circles at that famous meeting of the Geological Society of London on 18 December 1912? Perhaps. Yet I seriously

doubt that Sherlock's sage counsel would have been heeded, for, in 1912, the discovery of a creature such as "Eoanthropus" had been *logically* anticipated by anthropologists and paleontologists—or, rather, by certain influential ones, at least. In 1953, however, such a morphological monstrosity appeared anything but logical—hence Holmes' warning was heeded by Weiner. The moral is obvious.

WILLIAM L. STRAUS, JR.
Johns Hopkins University

Indians of the Northwest Coast. Anthropological handbook No. 10. Philip Drucker. Published for the American Museum of Natural History by McGraw-Hill, New York, 1955. 208 pp. Illus. $5.75.

The Coast Salish of British Columbia. Homer G. Barnett. University of Oregon Press, Eugene, 1955. 320 pp. Plates. $5.

The dramatic native cultures of the Northwest Coast of North America provide the materials for these two ethnographic accounts. Each is excellent in its way, Drucker furnishing a descriptive summary of the entire area, while Barnett offers a more detailed analysis of one of its local facets. The first is a handbook, designed for a wider public whose interest may be sparked by the ethnologic collections from the area in the American Museum of Natural History. The second is a carefully done descriptive monograph directed primarily to the professional.

The American Museum of Natural History is to be congratulated for its revival of its handbook series in ethnology. This is the second to be published within a year, the first being Lowie's *Indians of the Plains*, and it is to be hoped that others will appear shortly. New techniques of analysis, new data on the various ethnographic areas, as well as a reevaluation of earlier materials, permit a somewhat more lucid treatment of the native American cultures than was possible with the earlier and now outdated handbooks.

Drucker offers a concise summary of a culturally intricate area. Beginning with a survey of the land and the people, he turns to descriptions of subsistence and material culture and, thence, to social organization, religion, and ceremonial. The sensational art of these American Indians is also treated in detail. Although brief, the handling of the data is ethnographically wholly sound; attention is directed to the focal cultures of the area, but the marginal segments and the historical affinities of it are by no means neglected.

Drucker introduces his own previously published findings on the nature of social ranking on the Northwest Coast and effectively demonstrates the interrelationships between rank, wealth, titles, and the dramatic potlatching ceremonials, the well-known feasts involving wealth distribution to validate status. In line with the description of society, the author introduces the layman to the complexities of such societal forms as clan, moiety, lineage, and the like. The book serves rather well as an introduction to certain aspects of social anthropology and might do as a satisfactory supplementary classroom textbook. Unfortunately, it is too expensive for use of this kind.

As one moves down the British Columbia coast, the cultural vigor of such northern tribes as the Tlingit or Haida, or that of the famous Kwakiutl of the north of Vancouver Island, begins to be somewhat attenuated. Barnett has investigated some eight ethnic groups of the general Gulf of Georgia region, tribes speaking Salishan languages and occupying a somewhat marginal cultural position when they are considered against the tribes of the center and north. The resulting monograph is a detailed analysis of the shades of difference between the various tribes. Such an approach is always useful, demonstrating as it does the subtle differences that can arise between peoples who share the same cultural heritage.

The study is the result of careful and prolonged field work and reflects keen observation and insight. Taken together with Barnett's earlier treatment of the culture elements of the Salish, there is now available a full account of native culture of this southern segment of the Northwest Coast.

ROBERT F. SPENCER
Department of Anthropology,
University of Minnesota

The Antarctic Challenged. Edward R. G. R. Evans (Admiral Lord Mountevans). De Graff, New York, 1956. 247 pp. Plates. $4.50.

This book does exactly what the author states in his foreword it will do. It tells "in simple non-technical language the story of Antarctic exploration." As such, it is a book to be highly commended. It emphasizes the human and therefore the dramatic, rather than the scientific, aspect of Antarctic exploration.

Admiral Lord Mountevans was a member of the Second Scott Expedition, which ranks as one of the greatest exploring expeditions of all time. It is altogether natural that he should devote more

space to this expedition and to the others of that general period than to other aspects of the Antarctic story. Even so, for anyone who wants an introduction to the present status of Antarctic exploration, I commend this lively chronicle.

Perhaps the author's interest lags a little in the later years of exploration. He devotes attention to the First and Second Byrd Expeditions, to "Highjump," and to the notable Scientific Norwegian-British-Swedish Antarctic Expedition but somehow neglects any mention of the important work of the British expeditions and of the American Finn Ronne in the Palmer Peninsula land area in the last two decades.

This is a minor fault. On the whole, the book gives a good perspective of the achievements of the great pioneers such as James Cook, Durville, Wilkes, Ross, Shackleton, Amundsen, Scott, and Mawson, who perhaps has a more distinguished career in terms of scientific achievements than any other Antarctic explorer.

For anyone who wants an introduction to Antarctic exploration, I commend this book. It may be the last book that could rightly be titled *The Antarctic Challenged*. As of now the title is correct; but with the prospect of 11 nations establishing some 40 stations on the mainland of Antarctica and some 20 more on sub-Antarctic areas, it may be that the International Geophysical Year, which begins 1 July 1957, will make the title of this book obsolete.

LAURENCE M. GOULD

Carleton College

Oeuvres de Lavoisier. Correspondance. Fascicule I. René Fric, Ed. Editions Albin Michel, Paris, 1955. xviii + 252 pp. Illus.

The collected works of Antoine-Laurent Lavoisier (1743–94), commonly considered the creator of modern chemistry, were published in six volumes (Paris, 1862–93) at the expense of the government of France to honor one of her greatest sons. But this magnificent edition failed to include Lavoisier's correspondence. At long last this gap is now being filled. Letters to and from Lavoisier (and in some cases, about him) have been gathered together from all known sources, edited with loving care and annotated with meticulous attention to detail by René Fric, an engineer in charge of instruction at the Chemical Institute of the University of Clermont. Fric's edition of Lavoisier's correspondence is planned to comprise two volumes; hence the fascicle under review may be regarded as the opening portion of Volume VII in continuation of the aforementioned *Oeuvres de Lavoisier*.

The fascicle contains a preface by Louis de Broglie, winner of the Nobel Prize in 1929 for his work in theoretical physics and permanent secretary of the French Academy of Sciences, under whose auspices the fascicle was published. Its cost of publication was borne in part by the International Union for the History of Science and by UNESCO. The frontispiece is an excellent photographic reproduction of the well-known portrait of Lavoisier and his wife which was painted in 1788 by the foremost French artist of the time, Jacques Louis David. The only other illustration is a photocopy (pp. 222–223) of a letter written by Lavoisier in which he refers to his observation of the aurora borealis seen at Rheims on 24 October, 1769.

The correspondence contained in the present fascicle falls mainly into two groups. The first concerns a trip to Alsace-Lorraine undertaken by Lavoisier in 1767 with his geology teacher, who had been commissioned by the French king to conduct a mineralogical survey of that region. The second group deals with Lavoisier's activities in the tobacco department of the Farmers General, association with which later led him to the guillotine. But those tragic days were still far off. In this fascicle we see him displaying his indefatigable capacity for work, his uncanny shrewdness in judging human character, and his amazing adroitness in drafting reports and recommendations. One problem with which we watch him wrestle is the detection of retailers who adulterated tobacco by adding excessive water to it (a malpractice heavily emphasized in the indictment that cost him his life).

The letters are arranged in chronological order, a document which cannot be dated being printed at the end of the fascicle. The original spelling and punctuation are preserved throughout; because these texts have not been modernized, they should prove to be of value to students of the evolving orthography of the French language. They also provide some food for thought to those interested in various aspects of life in the 18th century, for they contain acute comments on such matters as speech, religion, costume, and diet. There is even a list of books which Lavoisier was quick to buy while he was in Strasbourg because he realized that they were not readily obtainable in Paris. But this fascicle will of course appeal mainly to two classes of readers: those concerned in general with the history of science and those intrigued by the fascinating biography of Lavoisier himself.

Since the editor will have ample opportunity on subsequent occasions to correct the errors committed in the present fascicle, he may wish to revise

some of his dates. In his Introduction (p. xi), for instance, he puts Lavoisier's visit to Alsace-Lorraine in 1768, whereas it actually took place in 1767. More serious is the mistake made in the title, according to which the correspondence begins in 1763, although the editor himself assigns the first letter to 1762. Lavoisier's father-in-law, who was executed with him in 1794, was born, not in 1676, as the editor would have us believe (p. 122), but in 1719. An inspection of mines took the traveler to Styria, not Syria (p. 116). Such easily remedied faults will not deter any reader who has access to Lavoisier letters unknown to the editor from communicating them to him. For the editor aspires to make his painstaking collection of his illustrious compatriot's correspondence as complete as possible.

EDWARD ROSEN

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Books Reviewed in SCIENCE

3 August

Learning across Cultures, J. Watson and R. Lippitt (Inst. for Social Research, University of Michigan). Reviewed by J. B. Casagrande.

The Meaning of Relativity, A. Einstein (Princeton University Press). Reviewed by P. G. Bergmann.

L'Evolution de la Lithosphere. I, *Pétrogenèse,* H. Termier and G. Termier (Masson). Reviewed by E. W. Berry.

Geometrical Optics, L. C. Martin (Philosophical Library). Reviewed by M. Herzberger.

The Systematic Identification of Organic Compounds, R. L. Shriner, R. C. Fuson, D. Y. Curtin (Wiley; Chapman & Hall). Reviewed by F. Ramirez.

10 August

Technical Education, P. F. R. Venables (Bell; Essential Books). Reviewed by D. H. Loughridge.

Biochemical Mechanisms in Inflammation, V. Menkin (Thomas). Reviewed by I. L. Bennett, Jr.

Rockets and Guided Missiles, J. Humphries (Macmillan). Reviewed by S. F. Singer.

Solid State Physics, F. Seitz and D. Turnbull (Academic Press). Reviewed by J. S. Plaskett.

Linear Feedback Analysis, J. G. Thomason (McGraw-Hill; Pergamon). Reviewed by J. Rothstein.

Principles and Practice of Field Experimentation, J. Wishart and H. G. Sanders (Commonwealth Bur. of Plant Breeding and Genetics, Cambridge, England). Reviewed by W. T. Federer.

17 August

Numerical Analysis, Z. Kopal (Wiley). Reviewed by A. S. Householder.

Combustion Processes, vol. II, *High Speed Aerodynamics and Jet Propulsion,* B. Lewis, R. N. Pease, H. S. Taylor, Eds. (Princeton University Press). Reviewed by E. S. Taylor.

Advances in Electronics and Electron Physics, vol. VII, L. Marton, Ed. (Academic Press). Reviewed by D. ter Haar.

Structure Reports for 1942–1944, vol. 9, A. J. C. Wilson, Ed. (Oosthoek's). Reviewed by G. A. Jeffrey.

American Foundations and Their Fields, W. S. Rich (American Foundations Information Service).

24 August

The Structure and Function of Skin, W. Montagna (Academic Press). Reviewed by L. H. Warren.

Pilot Plant Techniques of Submerged Fermentation, F. E. Paterno. (Fondazione Emanuele Paterno; distributed by Interscience). Reviewed by E. O. Karow.

Gestation, C. A. Villee, Ed. (Josiah Macy, Jr. Foundation). Reviewed by J. P. Greenhill.

Expanding Universes, E. Schrödinger (Cambridge University Press). Reviewed by C. N. Yang.

Engineering in History, R. S. Kirby, S. Withington, A. B. Darling, F. G. Kilgour (McGraw-Hill). Reviewed by A. P. Usher.

History of American Technology, J. W. Oliver (Ronald Press). Reviewed by A. P. Usher.

Yellow Fever Vaccination, World Health Organization (order from Columbia University Press). Reviewed by J. P. Fox.

31 August

Life, the Great Adventure, J. Rostand and P. Bodin (Scribner's). Reviewed by H. P. Papazian.

Genetics and Metabolism, R. P. Wagner and H. K. Mitchell (Wiley; Chapman & Hall). Reviewed by J. Lederberg.

The Dynamics of Living Protoplasm, L. V. Heilbrunn (Academic Press). Reviewed by J. J. Izquierdo.

A Handbook for the Identification of Insects of Medical Importance, J. Smart. [British Museum (Natural History)]. Reviewed by G. F. Ferris.

Clinical Electrocardiography, pt. I, L. N. Katz and A. Pick (Lea and Febiger). Reviewed by R. D. Pruitt.

An Illustrated Catalogue of the Rothschild Collection of Fleas (Siphonaptera) in the British Museum (Natural History), G. H. E. Hopkins and M. Rothschild [British Museum (Natural History)]. Reviewed by P. T. Johnson.

Elements d'Immunologie Générale, P. Gastinel, R. Fasquelle, P. Barbier (Masson). Reviewed by A. M. Pappenheimer, Jr.

ASSOCIATION AFFAIRS

AAAS BUILDING DEDICATION

To celebrate the completion of its new headquarters building, the Association will hold a special meeting in Washington, D.C., on 12 October 1956. In keeping with the nature of the Association, the program will be devoted to a scientific topic: "The uses and effects of atomic radiation." All members of the Association and of the interested public are cordially invited. The meeting will be held at the Carnegie Institution of Washington, 16th and P Streets, NW. At the conclusion of the meeting, the Association will hold open house at its new building at 1515 Massachusetts Avenue, NW.

PROGRAM

The Uses and Effects of Atomic Radiation

Chairman: Paul B. Sears, *President*

10 A.M.

Welcome
Caryl P. Haskins, *President, Carnegie Institution of Washington*

Radiation and the Human Body
Shields Warren, *Pathologist, New England Deaconess Hospital*

Radiation and Genetics
L. C. Dunn, *Professor of Zoology, Columbia University*

The Uses of Atomic Radiation and Energy
Lawrence R. Hafstad, *Vice President, General Motors Corporation*

2 P.M.

Social Implications of Atomic Radiation and Energy
Detlev W. Bronk, *President, Rockefeller Institute*

Current Research Findings
Willard F. Libby, *Commissioner, U.S. Atomic Energy Commission*

What We Most Need To Know
Laurence H. Snyder, *President Elect of the American Association for the Advancement of Science* and *Dean of the Graduate College, University of Oklahoma*

Dedication of the American Association for the Advancement of Science Building
Paul B. Sears, *President of the Association* and *Chairman, Conservation Program, Yale University*

New York Meetings of the AAAS, 1887–1956

The 123rd meeting of the American Association for the Advancement of Science this coming 26–31 December is also the seventh New York meeting. The previous New York meetings were held in 1887, 1900, 1906, 1916, 1928, and 1949. A brief comparison of the meetings indicates not merely the growth of the Association and the societies that participate but also provides an interesting indication of the shifts in emphasis in scientific research. The city's own development over 70 years, the influence of the times and of great personalities of the past are quite apparent in the programs and proceedings of these earlier conventions.

For some years the AAAS postponed a New York meeting because it was considered that the scientific population was smaller than in other centers. The first New York meeting, held 10–16 August 1887 at Columbia College, proved, however, to be a worth-while "experiment," and "the fear of hot weather [was not] fulfilled." There were 729 registrants from 31 states, Washington, D.C., and Canada, and 250 papers were read. Most of the sessions of the eight sections were held in Columbia's Hamilton Hall, then at 49 Street and Madison Avenue, and the "Programme" appropriately had a light blue cover. Samuel P. Langley, great physicist of the Smithsonian Institution, was president of the Association, which at that time had 1956 members; the retiring president's address, given by Edward S. Morse, summarized the many contributions of American zoologists to organic evolution during the preceding decade. F. W. Putnam was permanent secretary of the Association. The general chairman was F. A. P. Barnard, Episcopal clergyman and astronomer, president of Columbia, who had been president of the Association 21 years before, in 1866. The local committees of more than 500 prominent persons included Chauncey M. Depew, J. Pierpont Morgan, and Theodore Roosevelt.

Among the several participating organizations, the Torrey Botanical Club sponsored a field trip on Staten Island and a reception on the campus. The evening lecture, "The heart of Africa," by Henry Drummond of Glasgow, Scotland, was followed by a general reception to the Association with the New York Academy of Sciences as host.

After an interval of 13 years, the second New York meeting, 25–30 June 1900, was held at Columbia University, which by then had been moved to its present location on Morningside Heights. The headquarters hotel was the Majestic at 72 Street and Central Park West, where the rates were "$1.50 and upwards." The local committee, composed of all AAAS members within a 25-mile radius of the city and headed by J. J. Stevenson, together with the AAAS Council, announced their intention ". . . to reduce the excursions and other social features . . . so prominent at many previous meetings, to such a point that there [would] be no possible interruption to the scientific sessions. . . ." In this they were but partly successful. Fifteen societies participated, including two that had also met the two preceding days, and 253 papers were read. Although the official registration was only 447, it was estimated that some 300 other scientists were present, making the combined attendance of the AAAS and the societies about as large as usual. R. S. Woodward, Columbia mathematician, was the 52nd president of the Association, and L. O. Howard, eminent entomologist, was the permanent secretary. The retiring president's address, "Rhythms and geologic time," was given at the American Museum of Natural History by Grove K. Gilbert, who had succeeded his fellow geologist, Edward Orton, the third AAAS president to die in office. At this meeting resolutions were passed in favor of preserving the redwoods of California, creating a national forest reserve in the southern Appalachians, and establishing a National Bureau of Standards. The Council's action in making *Science*, owned and edited by J. McKeen Cattell, an official journal of the Association, included with the dues, was destined to result in a rapid increase in AAAS members from the static figure of 1925 that year. A new section on physiology and experimental medicine was authorized.

The third New York meeting, once more at Columbia, was held 27 December 1906 to 2 January 1907. Again J. J. Stevenson served as general chairman, and J. McKeen Cattell was secretary of the local executive committee. The full local committee consisted of all members within a radius of 50 miles. The permanent secretary, L. O. Howard, asked for the early submittal of titles because an effort was to be made to publish the entire program prior to the opening date; many titles, however, came in late. Hotel headquarters were at the Belmont across from Grand Central Station; the special rates to AAAS members for single rooms were $3 with bath, $2.50 without. There was now a subway, the trains of which, in this pre-shuttle period, ran directly to Columbia University. In the 6 years since the previous New York meeting, the Association, with 4498 members, had more than doubled in size; 19 societies participated, and 360 papers were read. The authors included virtually all of the prominent scientists of the day and many younger men who were destined to achieve lasting recognition. Genetics was becoming a science in its own right. In most fields, experimental work was taking the lead over purely descriptive observations.

At this meeting, there were 934 who registered,

and the total attendance was "conservatively estimated" at not less than 1500 scientists, which would make it the largest AAAS meeting up to that time. W. H. Welch, noted medical scientist from Johns Hopkins, took the gavel of the presidency of the Association from Calvin M. Woodward, mathematician, whose retiring address was "The science of education." Principal of the numerous social events was the reception which followed, given by the president of Columbia, Nicholas Murray Butler, in Earl Hall. Another feature of the meeting was the unveiling of ten busts of pioneer American scientists which were presented by Morris K. Jesup to the American Museum of Natural History. In addition the sessions at Columbia and at the museum, some sessions were held at the New York Botanical Garden and in the new building of the Rockefeller Institute for Medical Research. A new section on education was approved.

It was at the third New York meeting that the present Botanical Society of America was founded, 27 Dec. 1906, by the merger of several botanical societies. The golden anniversary of the society will be appropriately celebrated at the 1956 New York meeting on the same day in December.

The Association's 69th meeting—the fourth New York meeting—26–30 December 1916, was described by J. McKeen Cattell as "the largest and most important gathering of scientific men hitherto held in the country or elsewhere." Twelve sections and 52 societies participated, and 1252 papers were read. The registration was about 2100, and the total attendance was estimated as perhaps 8000; in 10 years AAAS membership, approximately 11,000, had more than doubled. Although the meeting was held principally at Columbia and the American Museum of Natural History, numerous other sessions were widely scattered because of the preferences of so many societies. In addition to the Belmont, once again AAAS headquarters, the Astor, Waldorf, Biltmore, and seven other hotels were used.

World War I was in its third year in Europe, and a number of the programs reflected the emotions of the period. The symposium of the American Society of Naturalists, for instance, was "Biology and the national existence," and several vice-presidential addresses referred to preparedness. Two public lectures were given: A. A. Noyes of Massachusetts Institute of Technology spoke on "Nitrogen and Preparedness," and Simon Flexner, in this epidemic year, spoke on "Infantile paralysis and the public health." The AAAS retiring president's address, "The nebulae," was given by astronomer W. W. Campbell; president Charles R. Van Hise, prominent geologist, presided. Henry Fairfield Osborn was general chairman, and L. O. Howard was still permanent secretary. A feature of the meeting, at the Engineering Building, was the symposium, "The interrelations of engineering and science," jointly sponsored by the ASCE, AIME, ASME and AIEE. The scientific exhibits in University Hall attracted much attention.

During the 1920's the AAAS inaugurated a plan to make every fourth meeting, to be held in a major city, a particularly large and diversified one. The fifth New York meeting of 26 December 1928 through 2 January 1929 was the second in this pattern. With a registration of 3935—AAAS membership then stood at 16,328—and several thousand other scientists who attended, this 85th meeting of the Association was exceeded only, and then but slightly, by the 1924 meeting in Washington, D.C. On the programs of the 15 sections and 45 societies that participated were more than 2000 papers. The approximately 250 sessions once more were held principally at Columbia University but the American Museum of Natural History, the Metropolitan Museum of Art, the headquarters of the American Geographical Society, and the Engineering Building were also used rather intensively. Despite the size and complexity of the meeting, however, it was exceptionally well organized and successful. Henry Fairfield Osborn, president of the American Museum of Natural History and current president of the Association, took an active personal interest, and the 11 local committees were under the able leadership of George B. Pegram of Columbia. The General Program of 344 pages was edited for the Association by Sam F. Trelease of Columbia; the permanent secretary of the AAAS, Burton E. Livingston, was ably assisted by Sam Woodley, in charge of meeting details, by H. S. Kimberley as exhibit manager, and by Austin H. Clark, who directed the Press Room.

The meeting was memorable for an unusual number of social events and general evening sessions, which included the AAAS presidential address of A. A. Noyes, "The story of the chemical elements"; the seventh annual Sigma Xi lecture, "What is light?" by Arthur H. Compton; and an address, "The scientific retrospect," by the noted astronomer, H. H. Turner, official representative of the British Association for the Advancement of Science. Other evening speakers were C. E. K. Mees, Charles P. Berkey, C. A. Kofoid, Franz Boas, W. M. Wheeler, and Harlow Shapley. Daily teas at Columbia, five receptions at the American Museum of Natural History, a Sunday largely devoted to tours of the city's scientific institutions, and an endowed, complimentary concert by the New York Philharmonic Orchestra added to the enjoyment of the registrants. The sixth AAAS Thousand Dollar Prize was awarded to Oliver Kamm for a paper on pitui-

tary hormones, which was read before Sections C and N. Thirty-six commercial exhibitors participated in the Association's fifth official Exhibition of Science.

The sixth New York meeting—the 116th AAAS annual meeting—26–31 December 1949, was delayed for 21 years through a combination of circumstances that included World War II and the Centennial Meeting of the preceding year. It was by far the largest meeting in the long annals of the Association. Sixteen sections and 61 societies—including groups in the social fields which seldom meet with the AAAS—participated, and many of these experienced a larger attendance than they anticipated. There were 398 sessions (294 with projection) in 63 rooms, one-third of these on the campus of Columbia University, the others in the same Penn Zone hotels as the 1956 meeting. Registrations were 7014, but very probably at least another 12,000 attended some phase of the meeting. In addition to every section of the nation, 30 foreign countries were represented. The American Museum of Natural History was the site of a particularly pleasant Biologists' Smoker. The 102nd president of the Association was Elvin C. Stakman. The AAAS presidential address, "Ten million scientists," which pointed out the pleasures and importance of scientific observations by laymen, was given by Edmund W. Sinnott. Joseph W. Barker was general chairman, and the administrative secretary, who had just succeeded F. R. Moulton, was Howard A. Meyerhoff. It was the first AAAS meeting for the arrangements of which I was responsible.

Among the outstanding features of the meeting were the address on infrared studies by G. B. B. M. Sutherland, official representative of the British Association; the Sigma Xi address, "Evolution in the tropics," by Th. Dobzhansky; the first RESA address, "Recent advances in nuclear physics," by John R. Dunning; the lecture and film of the National Geographic Society on Arnhem Land by Frank M. Setzler; and the Josiah Willard Gibbs lecture of the American Mathematical Society by Norbert Wiener, "The mathematics of sensory prosthesis." Nuclear physics and engineering, the adrenal cortex, and television were the subjects of prominent and timely symposia. The 22nd AAAS Thousand Dollar Prize was awarded to Armin C. Braun for a paper on the tumor-inducing principle in crown gall, which was read before the American Phytopathological Society; the winner of the sixth Theobald Smith award, given by Eli Lilly and Company, was Seymour S. Kety of the University of Pennsylvania School of Medicine. The 26th edition of the Annual Exposition of Science and Industry, with 68 commercial exhibitors, was substantially larger than ever before. A feature, new since the previous meeting—the AAAS Science Theater—was popular throughout the period.

In summary, the records of all previous meetings in New York do not fail to mention the warm spirit of hospitality and interest in the Association and its work shown by the people of this great city.

In the 7 years since the last New York meeting, the membership of the Association has grown from 44,947 to more than 51,000, and there are now 264 organizations affiliated with the AAAS (with action on seven others pending this December) compared with the 214 in 1949. The two journals of the Association have gained in circulation correspondingly; *Science,* with its size and format changed in July 1955, has increased its text coverage more than 50 percent. Some 20 symposium volumes have been published since the sixth New York meeting, and nine others are in press and in preparation at this time.

At this year's meeting Section P–Industrial Science, activated in 1951, will hold its sixth program, and Section Nd–Dentistry and Section Np–Pharmacy have had full status as independent sections since 1954. In addition to its usual activities, the AAAS has undertaken new responsibilities including the sponsorship of the International Arid Lands Meetings in New Mexico in 1955. In the current Science Teaching Improvement Program, supported by a grant from the Carnegie Corporation of New York, and the continuation of the Traveling Science Libraries for Small High Schools, financed by the National Science Foundation, the Association has demonstrated its abiding concern for the advancement of science, of science education, and of a better public understanding of the importance and promise of science.

In 7 years, the character of the annual meeting, too, has evolved in line with the thinking of the Arden House Conference. Added to the familiar pattern of many participating societies with their numerous sessions for short reports of current research, the 18 sections of the Association provide strong programs of invited papers in each of the principal fields of science. These symposia, traditionally associated with the AAAS meetings and often aggregating half a thousand speakers, have shown an increased emphasis on important interdisciplinary subjects. The distinguished evening lectures of Sigma Xi, RESA, the National Geographic Society, and Phi Beta Kappa have continued without interruption; the recurrent conferences on academy of science problems, on scientific manpower, and on scientific and technical editorial

problems have developed, and meet, a continued interest.

Among the societies participating in 1956 but not present at the 1949 New York meeting are the American Association of Clinical Chemists, American Association of Hospital Consultants, American Astronomical Society, American Documentation Institute, American Psychiatric Association, American Society of Range Management, Association for Computing Machinery, Entomological Society of America, History of Science Society, New York Academy of Sciences, and the Society of General Physiologists. The Society for the Study of Evolution, participating in December 1956, last met with the AAAS in 1949.

Other features of this seventh New York meeting include the two-afternoon general symposium, "Moving frontiers of science," organized by the relatively new standing Committee on AAAS Meetings; the silver anniversary of the AAAS–Gordon Research Conferences—a dinner with an address by Glenn T. Seaborg; the centennial anniversary symposia honoring Sigmund Freud (in the program of the American Philosophical Association) and commemorating Kraepelin (in the program of the American Psychiatric Association); Section G's symposium in honor of the golden anniversary of the Botanical Society of America; and the tenth annual Junior Scientists Assembly—a carefully planned afternoon for high-school science students, with the theme, "What makes a scientist?" The AAAS Science Theater, with its showing of the latest foreign and domestic scientific films, and the 33rd AAAS Annual Exposition of Science and Industry—a large-scale exhibit of the tools and techniques of science—will be well worth the consideration of all who attend.

RAYMOND L. TAYLOR
Associate Administrative Secretary, AAAS

Erratum: In the "Preliminary announcement of the seventh New York AAAS meeting," July issue, page 51, the name "Society of Vertebrate Zoology" was mistakenly given as one of the cosponsors of the symposium on "Biotic communities in the past and today." The correct name of the society referred to is Society of Vertebrate Paleontology.

Courses for Science and Mathematics Teachers

During March 1956 the AAAS Science Teaching Improvement Program mailed a questionnaire to 809 4-year colleges asking for information concerning the contributions of colleges to the improvement of the teaching of science and mathematics in high schools. The questionnaire was directed toward the in-service, or "emergency" problem.

The colleges selected were those that had enrollments in excess of 500, since it was felt that this group would train most of the teachers. Questionnaires have been returned by 727 of the colleges, which are in the 48 states, Alaska, Hawaii, Puerto Rico, and the District of Columbia. Only 39 institutions with enrollments greater than 1000 have not replied.

Teachers are often assigned science or mathematics as a second, third, fourth, or fifth teaching field, after having been granted certification on the basis of a bachelor's degree earned in other areas. When these teachers attempt to return to college to take graduate courses in science and mathematics, they often find that the prerequisites for such courses preclude their enrollment. Usually the advanced degree in professional education is the only alternative.

The first question on the STIP questionnaire was directed toward determining the number of colleges that meet this problem by offering special subject-matter courses for graduate credit and for teachers only. Eighty-two of the 727 colleges answered *Yes* to all three parts of the question. Some of the colleges offer courses with no prerequisites; however, nearly all special subject-matter courses name a few prerequisite courses.

A second question was directed toward establishing the number of colleges that offer correspondence courses for teachers of science and mathematics. Only 28 courses in mathematics are offered by the responding colleges. There are 66 courses in science, with distribution as follows: astronomy, 8; biology, 17; botany, 6; chemistry, 7; geology, 5; physics, 7; physiology, 7; zoology, 5; other science, 4. Of the 727 colleges reporting, 679 do not offer correspondence courses in science or mathematics. Professional education courses are offered by 39 of the colleges.

A third question dealt with extension courses given for teachers off the campus. The problem of graduate credit is more commonly present here, since more scheduled activity is required of the student. It was not determined whether or not graduate credit is allowed for the courses summarized in the following data: astronomy, 15; biology, 82; botany, 30; chemistry, 33; geology, 37; mathematics, 113; physics, 31; physiology, 15; zoology, 28; other science, 30; professional education, 193. Among the colleges responding, 495 do not offer extension courses in these subjects.

Other types of assistance offered by colleges to

high-school teachers are as follows: 419 colleges provide assistance in classroom activities and career guidance; 182 hold workshops; 155 convene summer institutes; 165 send representatives to in-service conferences; 99 grant scholarships; and 99 offer lecture series to which teachers are invited. These programs for the in-service teacher sometimes provide special bases for awarding college credit. When outside grants are given, the grantor may specify that graduate credit be given to those who desire it.

The provision of scholarships for teachers by 99 of the colleges is encouraging. These scholarships range from a partial remission of fees to a sizable stipend, but in all cases they are a direct aid, because they improve the over-all salary situation. They not only make it easier for the teacher to attend school and increase his subject matter competence but also provide increased qualifications for raises based on the state salary scale.

I. E. WALLEN
JOHN R. MAYOR
Science Teaching Improvement Program, AAAS, Washington, D.C.

Secondary-School Enrollments

The AAAS Science Teaching Improvement Program has issued a report on secondary-school enrollments in the 2-year period 1953–55. The study involved 1.15 million students in 39 states and 80 school systems. It is hoped that the results are representative of the situation in the country as a whole, for if this is so, the downward trend in science and mathematics enrollments has ceased, and enrollments in these courses are increasing at a faster rate than total enrollments.

Science enrollments showed slight annual increases in 47 school systems in both 1954 and 1955, rising in 1955 to 55.8 percent of the year's enrollment. In 1953 the mathematics enrollment in these same school systems was 50.4 percent of the total enrollment (470,477 students), and in 1955 the percentage rose to 51.6 percent (494,947 students). These increases were in addition to an over-all increase in enrollments of 5.2 percent (24,470 students) during the 2-year period.

Of the 80 school systems, those that showed the greatest increase in science enrollments in 1955 over 1953 are Little Rock, Ark., which had a 33.5 percent increase within a student population of 2593; Muncie, Ind., 47.6 percent increase and 1870 students; Fort Thomas, Ky., 39.2 percent increase and 400 students; Bangor, Me., 33.6 percent increase and 486 students; Oneida, N.Y., 56.3 percent increase and approximately 250 students; Grand Rapids, Mich., 50.5 percent increase and 645 students; and Platteville, Wis., 52.7 percent increase and approximately 500 students.

The schools that had a large percentage of increase in mathematics enrollments included Midwest City, Okla., 86.7 percent increase and 790 students; Belleville, Ill., 79.6 percent increase and approximately 1500 students; Opelousas, La., 46 percent increase and 486 students; Tampa, Fla., 43.7 percent increase and approximately 2000 students; Omaha, Neb., 35.7 percent increase and approximately 1500 students; Fort Thomas, Ky., 34.9 percent increase and 400 students; New Albany, Ind., 32.1 percent increase and approximately 1800 students; and El Centro, Calif., 30.3 percent increase and 1080 students.

Nine of the 80 school systems reported a decrease in the number of science enrollees between 1953 and 1955. The decreases ranged from three fewer students in Harrisburg, Pa., to 583 fewer in Philadelphia, with a total enrollment decrease of 899 science students.

Ten of the 80 school systems reported a decrease in the number of mathematics students. The total decrease was 1290 mathematics students; this loss ranged from 655 less in New York to one less in Beloit, Wis.

Only 14 schools sent data on enrollments in individual science courses, and in these the total enrollment increased by 8.4 percent during the 2 years. The percentage of those taking biology was the same in 1955 as in 1953, but physics enrollments increased by approximately 17.5 percent, chemistry enrollments increased by 22.4 percent, and general science enrollments increased by 24.1 percent. Although the number of students (122,673) is small in this group, it is interesting that physics, chemistry, and general science enrollments rose more than twice as fast as total enrollments, while biology held its 1953 position.

In mathematics, geometry enrollments in eight schools were up an average of 12.4 percent, trigonometry enrollments in four schools were up 17.9 percent, and in all 14 schools algebra enrollments were up an average of 29.6 percent. Although these reports indicating small increases in the percentages of secondary-school students interested in science and mathematics are encouraging, it is obvious that much remains to be done if the anticipated scientific-personnel needs of the next decade are to be met.

I. E. WALLEN
JOHN R. MAYOR
Science Teaching Improvement Program

Theobald Smith Award Judges

The president of the AAAS, Paul B. Sears, with the unanimous approval of the executive committee, has appointed the following judges for this year's Theobald Smith award: Edward A. Doisy, Sr., professor of biological chemistry, St. Louis University School of Medicine; Charles B. Huggins, professor of surgery and physiology, University of Chicago; Ancel Keys, professor of physiological hygiene, University of Minnesota; and Cecil J. Watson, professor of medicine, University of Minnesota.

Irvine H. Page, director of research of the Cleveland Clinic and vice president of AAAS Section N–Medical Sciences, serves *ex officio* as chairman of this committee, and Allan D. Bass, professor of pharmacology at Vanderbilt University School of Medicine and secretary of Section N, will serve as committee secretary.

Socio-Psychological Prize Judges

Judges for the 1956 AAAS Socio-Psychological prize essay contest are Fred L. Strodtbeck, University of Chicago Law School; Raymond V. Bowers, chief of the social science plans division, Air Force Personnel and Training Research Center, Lackland Air Force Base; and Kimball Young, department of sociology, Northwestern University.

Junior Academies of Science

With a serious scientific manpower shortage facing our country it is obvious that the young scientist must play a very important part in the world of tomorrow. The junior academy of science movement is one of the active interests of the AAAS Science Teaching Improvement Program in its work of discovering and developing scientific talent in American youth.

Twenty-five states and three cities report having a junior academy of science. The academies may have science-club memberships, individual memberships, or be organized along either of these patterns. Although the junior academies are primarily an activity at the high-school level, 18 of them include junior-high students and one has only junior-college students.

The junior academies are often financially supported by means of dues, registration fees, sponsoring senior academies, bequests, memorials, grants, or other sources. The academies are usually directed by an advisory committee that is under the control of the senior academy. Students occupy the major offices in 18 of the organizations.

All but two of the academies have at least one meeting a year with the senior academy. The contact with the senior scientists of the state is usually the most inspiring part of the program, and the senior scientists are always impressed by the talent observed among the juniors.

The presentations of student research are the most important part of the junior academy meetings. The research may be presented as an exhibit, a demonstration, or a paper. The projects are not always technically accurate and students do not have the scientists' knowledge that would lead them to limit their fields of endeavor. Consequently, some highly technical scientists have expressed their doubt regarding the value of projects, but one needs only to talk to junior members at their meetings to recognize the value of such projects in the stimulation of scientific thinking at the high-school level.

Lectures are frequently given at the junior academy meetings by distinguished state or national scientists who are leaders in their fields. Most of the junior academy programs include trips to museums, industrial plants, and other places of scientific interest.

The stimuli of attending meetings, sharing ideas on projects, and meeting senior scientists make attendance at junior academy meetings a coveted trip for students, but in many states further awards are given to participants. These awards may be cash, certificates, scholarships, loving cups, ribbons, plaques, or subscriptions to science magazines. Two honorary annual memberships in the AAAS are available to each academy. In four states outstanding teacher-sponsors are also rewarded by citations, pens, keys, or summer scholarships that may be used for graduate work.

Several of the junior academies publish announcements, information, scientific articles, book reviews, or similar features. The junior academies that have such publications usually maintain a higher level of interest than the others. A few junior academies maintain speaker and counselor committees by means of which college and industrial personnel are made available as speakers and advisers for science clubs, classes, or other meetings.

In some states weekly radio and television programs are prepared. In others Kodachrome slides of projects are available for loan. Coprojects of many kinds with senior scientists have proved attractive to many students.

Junior academies often cooperate with state

science education sections to aid in bringing new fields of science to the attention of high-school teachers. A recent education survey points to the secondary-school science teachers as those most responsible for the early identification and channeling of potential scientists. The junior academies of science are designed to assist the secondary-school teacher in appealing to superior students and guiding them along the pathway of science. There is a definite place for these junior science activities in helping to alleviate the manpower shortage.

THELMA HEATWOLE
Woodrow Wilson Educational Center, Staunton, Virginia

AAAS Finances: Report for 1955

The Association's financial records are kept in two separate accounts. One—the Operating Account—shows the expenses of running the Association's ordinary, continuing activities and the income that is devoted to those activities; the other—the Investment Account—includes funds that have been given to the Association to endow prizes, to pay the cost of maintaining the membership of emeritus and life members, and to support research.

Operating Account

Operating receipts totaled $602,420.68 during 1955. This income was divided as follows:

Annual dues paid by members	$288,808.62
Extra payments by members who wanted to receive both *Science* and *The Scientific Monthly*	18,927.37
Money transferred from the Investment Account to pay for subscriptions for emeritus and life members	3,330.00
Journal subscriptions from nonmembers	60,625.97
Sales of individual copies of journals	2,476.31
Advertising in *Science*	142,054.25
Advertising in *The Scientific Monthly*	11,615.79
Sale of symposium volumes	24,023.59
Miscellaneous sales, including journal binders, Association emblems, and microcard edition of *Science*	4,351.99
Annual meeting: registration fees, exposition space, advertising in program, and contributions	27,470.61
Income from investment of funds not needed in checking account	13,751.67
Miscellaneous other income	4,984.51
Total	$602,420.68

These receipts amounted to $8744.61 more than the operating expenses. This amount, therefore, was the Association's surplus for the year. The principal items of expense were:

Printing and editing *Science*	$267,137.80
Printing and editing *The Scientific Monthly*	77,282.85
Editorial Board expenses and honoraria	7,136.20
Cost of selling advertising in the two journals	36,101.46
Printing and editing symposium volumes	21,736.91
Meetings of boards and committees	7,942.44
Contribution to Scientific Manpower Commission	1,000.00
Recruitment of new members	15,223.22
Expenses of the annual meeting	28,905.01
Press service for annual meeting	4,687.22
Allowances ($1 per member) to Pacific, Southwestern and Rocky Mountain, and Alaska Divisions	5,740.00
Expenses of AAAS sections	4,584.05
Administrative staff salaries and general expenses	82,340.77
Building maintenance and rental of temporary quarters during construction of new building	19,586.89
Social security, insurance, and retirement provisions for staff members	10,860.51
Miscellaneous other expenses	3,410.74
Total	$593,676.07

Comparison of 1955 with 1954

Receipts in 1955 were $7971.92 smaller than in 1954. The smaller total was chiefly accounted for by two facts: the Association derived $15,801.27 less income from the 1955 annual meeting in Atlanta than from the 1954 annual meeting in Berkeley; and the 1955 income from rent of the houses owned but not occupied by the Association was $5029.48 smaller in 1955 than in 1954. Early in 1955 those houses were torn down to make way for the new headquarters building.

Operating expenses in 1955 were greater by $27,933.28 than they were in 1954. The largest increases were in the cost of publishing the Association's journals and in the cost of housing the Association's offices. In 1955 we spent $22,703.52 more than in 1954 in editing and printing *Science* and *The Scientific Monthly*. The necessity of renting office space for most of 1955 meant that housing cost $10,354.22 more than it had in 1954.

Balance Sheet

All the foregoing deals with the operating funds of the Association for the entire year of 1955. At the end of 1955, the Association's balance sheet showed the following assets:

Cash	$261,533.37
Money owed to the Association	21,991.41
Money invested in short-term and easily convertible securities (e.g., U.S. Treasury bills and Building and Loan Association deposits)	408,527.64

Value of real estate owned by the Association	153,297.20
Construction account for new building	479,527.42
Total assets	$1,324,877.04

Offsetting these assets, as they always must in a balance sheet, were liabilities, as listed below:

Accounts payable to others	$ 104,039.31
Unexpended portions of grants from Carnegie Corporation and National Science Foundation	97,283.93
1956 dues received, but for which members had not yet received journals or other services	223,884.14
Subscriptions to 1956 issues of journals	54,059.04
Money earmarked for construction of new building	476,756.79
Unallocated funds, available to the Association for operating expenses, payment on new building, or other purposes	368,853.83
Total liabilities	$1,324,877.04

Grants Received in 1955

Among the liabilities shown above is an item of $97,283.93 which represented the unexpended portions of grants made to the Association during 1955. Expenses, under these grants, were not listed as expenses, nor were the grants listed as income, for the grants represent special projects undertaken by the Association. They are not part of the regular operating funds, even though the purposes for which the grants were received are part of the Association's program of activities; including them in the Operating Account would give a grossly distorted picture of the Association's income and balance for the year. The grants received are briefly accounted for below.

During 1954 and 1955 the Association received grants totaling $24,770.89 to pay the expenses of the International Arid Lands Meetings held in New Mexico in April and May of 1955. The expenses of the meetings totaled exactly the same amount, and the nice balance is explained by the fact that what would otherwise have been a deficit of $1070.89 was canceled by contributions from generous supporters in the area in which the meetings were held. The other contributions to support the meetings were $10,000 from the National Science Foundation, $10,000 from the Rockefeller Foundation, $3000 from UNESCO, and $700 from the Department of State.

The National Science Foundation made three other grants to the Association during 1955. One, of $2820, was to finance a conference on the education of science teachers. The conference cost $1762.82 and the remaining $1057.18 was returned to the National Science Foundation. A second grant, of $2300, was for the purpose of holding a conference on the science training of talented students. The conference cost $1324.41 and the balance of $975.59 was returned to the National Science Foundation.

The other grant from the National Science Foundation was for $23,250 and was given to enable the Association to develop traveling libraries of books about science and scientists that were rotated during the academic year among 66 small high schools scattered throughout the United States. As of 31 December 1955 there remained $8946.70 in this account. This money was retained by the Association for the purpose of paying expenses of the program that had not yet been incurred by the end of 1955.

From the Carnegie Corporation, the Association received $100,000 on 1 July 1955. This amount was the first third of a grant of $300,000 to support the AAAS Science Teaching Improvement Program. Since the program did not get actively under way until September of 1955, there was a substantial balance of $88,337.23 on 31 December.

Members and other friends contributed $32,775.88 to the Association's building fund during 1955.

Not included in the above accounting are grants of $10,000 each from the Rockefeller Foundation and the National Science Foundation that were made to the Gordon Research Conferences.

Investment Account

To keep them separated from operating funds and grants for special activities, the Association holds its endowment and investment funds in a separate Investment Account. Although the Association has some leeway in the handling of these funds, ordinarily the income is used and the principal is left intact. Disbursements during 1955 were as follows:

Income allocated to Gordon Research Conferences	$1015.36
Newcomb Cleveland prize	1000.00
Socio-psychological essay prize	1000.00
Grants to academies of science to use in support of research	4612.28
Transferred to operating account to pay for journal subscriptions for emeritus and life members	3300.00
Expenses of managing investments	1742.75

The accompanying statement of receipts and disbursements from this account also shows that $273,268.50 was spent for the purchase of securities. All but a small fraction of this amount was derived from the sale of other securities.

At the end of 1955 the Investment Account showed assets of $435,340.03, of which $36,502.75 consisted of endowment for the Gordon Research Conferences and $398,837.28 was for the other purposes mentioned above. The funds in the Investment Account were distributed as follows:

	Book value	*Market value*
Cash	$ 16,334.46	$ 16,334.46
U.S. Government bonds	143,386.51	138,937.00
Other bonds	75,932.25	75,600.00
Preferred stocks	58,908.59	60,375.00
Common stocks	140,778.22	215,763.00
Total	$435,340.03	$507,009.46

Prior to 1955 a larger fraction of this account had been invested in Government securities. During 1955, with the advice of the investment firm of Scudder, Stevens and Clark, the percentage in such securities was reduced while the percentage in corporate bonds and stocks was correspondingly increased.

Auditor's Report

A condensed statement of the Association's finances for 1955, prepared by the auditing firm of G. P. Graham and Company, is published herewith. Balance sheets and statements of receipts and expenditures are given separately for the Operating Account and the Investment Account. The term "Investment Account" has been adopted to replace the older name, "Treasurer's Account," but the auditor's report uses the older name. The portion of the auditor's report published here does not include funds for the Gordon Research Conferences.

DAEL WOLFLE

Executive Officer, AAAS

Washington 5, D.C.
25 May 1956

To the Council of the
American Association for the Advancement of Science
Washington, D.C.

We have examined the balance sheet of the Operating Fund of the American Association for the Advancement of Science as at 31 December 1955, and the statement of revenue and expenditures for the year then ended. Our examination was made in accordance with generally accepted auditing standards, and accordingly included such tests of the accounting records and such other auditing procedures as we considered necessary in the circumstances.

In our opinion, the accompanying statements present fairly the financial position of the Operating Fund of the American Association for the Advancement of Science as at 31 December 1955, and the results of its operations for the year then ended.

G. P. GRAHAM & COMPANY
By G. R. BOWERS

AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE OPERATING FUND
BALANCE SHEET AS AT 31 DECEMBER 1955

Assets

Current assets		
Cash on deposit	$ 258,033.37	
Imprest funds	3,075.00	
Accounts receivable	21,991.41	
Deposit with airline	425.00	
Investments	408,527.64	$ 692,052.42
Building accounts		
Investments	$ 975.87	
Real estate	153,297.20	
Building construction account	478,551.55	632,824.62
		$1,324,877.04

Liabilities

Current liabilities		
Accounts payable	$ 104,039.31	
Unexpended balances of grants		
Traveling High-School Science Libraries	8,946.70	
Science Teaching Improvement Program	88,337.23	$ 201,323.24
Deferred income		
Prepaid dues	$ 223,884.14	
Prepaid journal subscriptions	54.059 04	277,943.18
Building fund		226,756.79
Reserve fund		250,000.00
Unallocated funds		
Balance 1 January 1955	$ 360,109.22	
Add: Excess of revenue over expenditures	8,744.61	
Balance 31 December 1955		368,853.83
		$1,324,877.04

American Association for the Advancement of Science Operating Fund
Statement of Revenue and Expenditures for the Year Ended 31 December 1955

Revenue			
Dues			$288,808.62
Journals			
Subscriptions			
From Treasurer's accounts (Life, 50-year, and emeritus members)	$ 3,330.00		
Members' special subscriptions	18,927.37		
Nonmembers' subscriptions	60,625.97	$ 82,883.34	
Advertising		153,670.54	
Miscellaneous sales		2,476.31	239,029.69
Publications			
Binders		$ 1,268.53	
Symposium volumes		24,023.59	25,292.12
Atlanta meeting and exhibit			27,470.61
Income from investments			13,751.67
Miscellaneous			8,067.97
			$602,420.68
Expenditures			
Administrative and general expense		$ 82,340.77	
Building expenses		7,539.39	
Rental of temporary offices		12,047.50	
Board of directors		6,800.52	
Other committees		1,141.92	
Allowance to divisions		5,740.00	
Section expense		4,584.05	
Circularization—new members		15,223.22	
Meetings and exhibits		33,592.23	
Journals		387,658.31	
Publications		23,014.87	
Employees' retirement		8,008.13	
Social security		2,852.38	
Miscellaneous		3,132.78	593,676.07
Excess of revenue over expenditures			$ 8,744.61

Washington 5, D.C.
25 May 1956

To the Council of the
American Association for the Advancement of Science
Washington, D.C.

We have examined the balance sheet of the Treasurer's accounts of the American Association for the Advancement of Science as at 31 December 1955, and the statement of cash receipts and disbursements for the year then ended. Our examination was made in accordance with generally accepted auditing standards, and accordingly included such tests of the accounting records and such other auditing procedures as are considered necessary in the circumstances.

In our opinion, the accompanying financial statements present fairly the financial position of the Treasurer's accounts of the American Association for the Advancement of Science as at 31 December 1955, and the cash receipts and disbursements for the year then ended.

G. P. Graham & Company
By G. R. Bowers

American Association for the Advancement of Science Treasurer's Accounts
Balance Sheet as at 31 December 1955

Assets

Cash in bank		$ 16,334.46
Securities—at cost		382,502.82
		$398,837.28

Liabilities and Reserves

Liabilities		
Accounts payable	$ 62.00	
Academy grants	2,859.23	
Special academy grants	475.00	$ 3,396.23
Endowment funds		
For research	$174,289.52	
For general purposes	154,798.28	
For special purposes	66,353.25	395,441.05
		$398,837.28

AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE

TREASURER'S ACCOUNTS

STATEMENT OF CASH RECEIPTS AND DISBURSEMENTS
FOR THE YEAR ENDED 31 DECEMBER 1955

Cash balance 1 January 1955		$ 3,343.29
Receipts		
Income from investments	$ 12,368.16	
Redemption and sale of securities	270,606.76	
Life membership fees	4,650.00	
Gifts	2,919.69	
Special academy grants	450.00	
Deceased emeritus life membership fees	900.00	
Transferred to research fund income from UNESCO fellowship fund	8.50	
Refund of advance to Gordon Research Conferences	9,465.45	301,368.56
		$304,711.85
Disbursements		
Income allocated to Gordon Research Conferences	$ 1,015.36	
Securities purchased	273,268.50	
Newcomb Cleveland prize	1,000.00	
Socio-psychological prize	1,000.00	
Academy grants	4,362.28	
Special academy grants	250.00	
Emeritus life membership fees (From Jane M. Smith fund)	1,500.00	
Deceased emeritus life membership fees (To Jane M. Smith fund)	900.00	
Journal subscriptions (Life, 50-year, and emeritus members)	3,330.00	
Expenses	1,742.75	
UNESCO fellowship fund transferred to research fund income	8.50	288,377.39
Cash balance 31 December 1955		$ 16,334.46

An immense gain to philosophy, and the dissipation of an incredible amount of vague theorizing, must result when men have firmly fixed in their minds the true conception of Force as a mere abstraction which only has reality for us through its demonstrated Laws. People will cease to talk glibly of nerve-force, vital-force, etc., in explanation of phenomena not yet reduced to law; they will understand that only such effects as can be deduced from the known laws may be predicated of the unknown forces. Without being called upon to give up the advantages of having such abstractions as that of Force, they will escape the danger of metaphysical reliance on such generalized expressions.—G. H. LEWES, *Aristotle* (1864).